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A SCIENTIFIC QUARTERLY DEALING WITH EXTRASENSORY PERCEPTION, THE PSYCHOKINETIC EFFECT, AND RELATED TOPICS

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THE RELIABLE APPLICATION OF ESP

By Remi J. Cadoret

ABSTRACT: This article presents a new method to measure (and predict) the reliability of ESP performance before the trials are checked. The results of its first application are of borderline significance by the usual standards in psi research and the timely importance of the project warrants publication.

The author chose as the basis of his test the technique of locating concealed objects, using the automatisms or dissociated movements common to dowsing. Two types of target location were needed: one nearby for quick easy checking (this was called the index target), and the other more remote (which was called the test target). On the basis of the tests made with the index targets, the reliability of efforts to locate the test target was estimated and success was "predicted." The index target was a penny located under one of 25 square tiles arranged in a five-by-five matrix and the test target was a penny located in a similarly designed but larger plot, situated on the grounds of the author's residence. The trials intended for the plot of ground were carried out on a map with a five-by-five matrix similar in size and arrangement to the 25 tiles. Thus the performance for the subjects was essentially similar whether for the index target or the test target. A certain number of "diagnostic" trials were done on the index targets, followed by a limited number on the test targets in the same session. The results from the former were then used to estimate and predict the order of success expected on the latter.

The location-finding technique consisted of a two-step procedure. First the subject was instructed to choose the row in which he thought the coin lay, and after fixing upon a certain row, he was asked to indicate which square in the row he thought was the correct one.

The scoring of success was done, in the first two series, on both direct hits and near-hits, or displacement. The third series was scored for direct hits alone. The method used in making the predictions is described and its application to the third series is evaluated. In this series the index targets gave results of near significance and the prediction of the test targets based on them was significant.

Dr. Cadoret is a member of the staff of the Parapsychology Laboratory who is at present on leave for a term of service with the Medical Corps of the U. S. Air Force.—Ed.

I. A General Survey

While there have been many practices used through the ages in which psi has been believed to play a part, there has not yet been any appreciable success in the reliable application of the ability to the solution of problems. This apparent neglect of a challenging area of inquiry stems from the elusive nature of psi itself rather than from the technical difficulties of extracting the required information from the data. The mathematics of information and communication systems is well able to deal with the latter problem. The major obstacle to the practical use of ESP at the present time is the unreliability of its operation, at least in a recognizable form, under controlled observation. I shall survey briefly in the first section of this paper some of the previous attempts to make a reliable application of the capacity.

Probably the earliest approach to a method of practical application of ESP was tried in the early years of the Duke ESP investigations, both at the Parapsychology Laboratory and in exploratory investigations with which the Laboratory was in contact. Essentially it was a way of concentrating the small amount of scattered ESP activity so that a reliable quantity of information could be garnered from the data. The general method was as follows: A standard deck of ESP cards was prepared, with each card individually enclosed in a sealed opaque envelope and with the envelopes numbered. The individual cards, arranged in different order from one run to another, were used in a series of card-calling tests of ESP in the usual manner and the calls were recorded. After a specified number of trials had been made with the deck of enclosed cards, the call records were analyzed and the call most often made for a given card was chosen the most likely target symbol. The purpose of the test was, of course, to see whether the set of predictions derived by this method would prove to have a higher score average than the original calls themselves from which the derivation was made. All of these efforts were exploratory, and they did not reach the point of a systematic and exhaustive research. However, the method as far as applied was not successful; that is, it did not represent an effective way of concentrating the thinly scattered ESP hits which the significance of the over-all results indicated were present in the original records.

It was recognized at the time also that there were psychological difficulties inherent in the experimental procedure that were not adequately met at that stage of exploration; for example, the fact that the subject was told the nature of the experiment and realized that he was repeating trials on the same target material without being given the usual report of his scores after each run. Possibly because of this, there was in general a reduced scoring level which afforded a less adequate test than was desirable. Also, the method offers no control over the type of ESP manifestation (such as displacement or psi missing).

About fifteen years ago, A. A. Foster (2) experimented with a more complex method of obtaining practical information through ESP. In this method, which is the only one ever to appear in print, a question was selected to which the answer was either *yes* or *no*. To indicate the answer, the subject was given a deck of red and black cards in opaque envelopes and was instructed to sort them into two piles; a black area which meant *true* and a red area which meant *false*. If the answer to the proposed question was *yes* and the subject knew the answer (either sensorially or through ESP), then the task was to sort all the black cards into the black disposition area and to avoid doing so if the answer was *no*. Essentially the task was to match by ESP two like colors if the answer was *yes* and two unlike colors to designate *no*. In theory the answer, and even the question, could remain unknown to the subject, to the experimenter, or to both. Whether the subject could determine the correct answer to the question by ESP as well as indicate the answer by further exercise of ESP was a problem which Foster tried to answer empirically.

Unfortunately Foster found no evidence that ESP was operating when he tried this method on Indian children who had demonstrated ESP ability in other tests. However, this system embodies a feature which later methods have sought after: it contains a built-in indicator of ESP activity since to demonstrate a *yes* or *no* response successfully, ESP must operate. But at the same time, it contains no control over the direction of the response to the proposed question. A subject might successfully indicate *no* to a question whose answer was *yes*. Foster himself realized this when he wrote that "the sign of the response may reflect, not verity but the inmost desire

of the subject." A reliable method should not only contain a way to determine if ESP is operating but should also indicate the direction of the response to the particular question asked. A further requirement is that a method must offer some control over psi missing, consistent missing, displacement, and sundry other manifestations of ESP.

Most of the proposed methods of putting psi to practical use have been methodological or statistical innovations to control the direction and type of ESP manifestation in addition to concentrating the small amount of ESP present into reliable bits of information. I will present some of these developments even though experimental proof of their reliability is lacking to date. Their virtue lies in their theoretical implications and the glimpse they afford of the many problems of an elementary nature which we must solve to make practical ESP a reality.

One approach to controlling the direction of the response and the type of ESP manifestation was suggested by the proven uniformity of a subject's ESP performance shown during a given period of testing. Such uniformity has been demonstrated at many levels, from consistency of performance on tasks given at the same session (**6**) to the similarity of scoring within single runs as demonstrated by the decline effect. The possibility that consistency in performance both within the run and from run to run could be turned to dependable use was investigated at Duke about five years ago by S. J. Hallett (**4**). Hallett applied analyses of correlation and regression to various published data in which ESP was known to have occurred, and he found that it was perfectly feasible to use either random or systematic sampling of calls and runs to predict performance in the remaining data and thus identify the target cards with accuracy. He then tried to apply this general methodology to the reliable prediction of future events. In preliminary tests three groups of precognitive calls were made at one session by his subjects for targets to be selected in the future. Each call group was composed of trials distributed throughout the run in order to neutralize decline effects among the three groups; and from the performance on one group, hereafter called the *index trials,* Hallett hoped to be able to predict the rate of hitting on the other two groups and thence the identity of the targets themselves. Unfor-

tunately, in the several experiments tried, results did not exceed chance; that is, no ESP was demonstrated.

At least this method offers an index of the amount of ESP present—whether it be direct-target hitting, displacement hitting, or one of the other manifestations of ESP—and hence, offers the possibility of identifying an unknown target, hereafter called the *test target,* assuming a similarity of performance between the calls used for the index trials and the remainder of the calls made against the test targets (whose identity is to be determined from the former).

However, one can count on similarity of performance with the index targets and the test targets to occur only under similar conditions. Some of these conditions we know, others we can guess at, and still others we can barely conceive of with our limited knowledge. It seems obvious that the type of ESP used must be constant. For example, if clairvoyance is used to identify the index targets and precognition is used by the subject in responding to the test targets, we should not be surprised to find little correlation between the two. But it should be possible, by manipulating experimental conditions, to keep the type of ESP used constant in the two parts of the test.

The type of target is another condition which we might consider should be constant. Insofar as possible, the index targets should be similar to the test targets as a check on the unconscious distortion to which both sets of targets are exposed.

A further measure to insure similarity in conditions, and hence in performance, was demonstrated in Hallett's technique of keeping the subject purposefully unaware of whether a particular call was being made for an index target or a test target. Presumably, in this way we eliminate conscious bias, and the subject orients himself toward the proper target unconsciously.

But even if conscious bias is eliminated, there is still no control on the possibly more powerful unconscious factors, whose presence the psychopathology occurring in our everyday lives amply demonstrates. Can we depend on the unconscious to orient itself correctly toward the proper target? This certainly seems to place a great amount of confidence in an entity which most psychiatrists assure

us is both unscrupulous and undependable and out to get us, much like the devil of medieval days.

However, on theoretical grounds there is reason to believe that some confidence in the possibility of controlling unconscious bias may not be entirely misplaced. Very early in ESP investigations evidence was found which suggested that as a perceptual process, ESP is diametric in nature. By this is meant that ESP comprehends entire relationships in one action instead of piecing the relationship together from separate bits of information (**7**). For example, in a blind matching procedure in which the key cards are not known, subjects score about as well as they do in an open-matching procedure, in which the identity of the key cards is known, even though in the blind matching the ESP process apparently must ferret out more information. The reader can see how this applies to our situation: the diametric ESP process will theoretically be able to orient itself correctly toward the proper target with little further effort.

Some of the methods proposed have really leaned quite heavily on the diametric principle. The Foster work made use of it. Most of Hallett's methods relied on it. For example, in one of Hallett's techniques, a subject was presented with a number of multiple-choice questions. To some of them the answers were known, and these served as controls on index targets. The answer sheet was concealed within an envelope, making the process essentially a complex blind-matching procedure. This latter method never came to experimental trial, so we are unable to judge its efficacy. Whether techniques like this presume too much on the diametric nature of ESP is a question for research to solve. However, recent work by Soal with Mrs. Stewart (especially the telepathy-in-conjunction tests, in which two agents took an essential part) has tended to confirm the diametric hypothesis (**8**), and it appears that practical methods utilizing the possible diametric nature of psi are heading in the right direction.

There is another way in which elimination of conscious orientation might be profitable; that is in minimizing the dual-task effect such as that reported by Hallett (**3**). In these tests the subject had to identify two aspects of the same target, the symbol and its position in the row. If the subject regards such a test as two separate tasks, different scoring may result on each, as has been noted in

both ESP and PK experiments. The cause of this may be tied up with conscious motives, but it is equally as likely that the unconscious is an important factor.

At any rate, the conviction arises from a study of these various methods that, given similar performance for both index and test targets (which is presuming a great deal), statistical techniques of measuring the ESP present in the index trials and determining the identity of the test targets are feasible. One further point to consider in the statistical determination of targets to be ascertained from the index trials is that different methods may be tried empirically and tailored to give optimum results. If we might paraphrase a Biblical quotation applicable here: By their fruits ye shall know them—not by their roots. This freedom of exploration should be valid as long as we judge the final results *objectively.*

So far, we have been discussing the harnessing of ESP and have really been talking around the basic problem, which is the guided production of a recognizable manifestation of ESP. There have been many experiments relating ESP to personality factors and numerous other variables, but all that these studies have really measured is differences in the way ESP manifests itself. How and when it occurs is still unknown. If we wish to predict the future reliably or even be able to locate underground water, we must learn to produce ESP. All of the methods described above failed in practice at least partly because they gave no evidence of psi. Until the production of ESP is on a more reliable basis, the following suggestions for approaching the problem are justified and might incidentally shed light on the fundamental nature of ESP. "Crash" programs leading to ESP production might include an ESP game to be tried on enthusiastic audiences such as school children. Another approach easily overlooked because of its obviousness is the use of already skilled personnel who might use ESP, such as dowsers, when they are adaptable or trainable to the particular task.

In some of the many measurable physiological processes, correlates with ESP performance might be discovered, thus making the reliable application of psi easier as well as providing fundamental information as to the conditions under which ESP occurs. It is rather surprising that this approach has not been more often followed in our physicalistically oriented age.

Whatever the approach used to solve the enigma of volitional ESP control, we may be sure that much more effort will be needed, to judge by progress so far. Although the reliable use of ESP involves so many unknowns, the goal is worth the trials, both for utilitarian reasons as well as for the more intangible benefits which follow human self-discovery.

In these days when the prevailing philosophy is to use force to obtain one's ends, it may seem incredibly naïve to suggest that the ultimate weapon might prove to be knowledge and understanding of the basic motives and plans of our fellow-men. With psi suitably controlled we may have a means of penetrating the manifold barriers which separate us from each other, whether these be natural barriers of space and time or those which our societies and politics impose upon us.

II. A Pilot Study of a Method of Locating Spatial Events by ESP

Dowsing for hidden substances may represent a successful application of ESP to a practical problem of everyday living. The water dowser's claim of ability to produce results whether through phenomenal ESP ability, shrewd use of sensory aids (such as topography), or just because of the ubiquity of sub-surface water, is too well known to require further comment here. Discovery of the location of other minerals is an important area of human endeavor, which occupies hundreds of people in constant search for hidden wealth which is too often concealed by an indifferent nature with barely a clue to its whereabouts. The successful application of ESP on a firm scientific basis to facilitate such searches would mark an epoch.

As a starting point we can state in general terms that the need exists for an ESP method which can be applied to the solution of problems whose answer is some locus in space. This paper will detail the development of one such method, describe its testing in a simulated problem of location, and further witness its shortcomings as well as its minor triumphs.

One approach to this problem was suggested by the work of Fisk (**1, 5**) in Great Britain with what are called Clock Cards. On these cards is printed the face of a clock whose single hand points

to one of the twelve hours. These investigations have demonstrated that in addition to direct target hour hits, calls tended to center about the target hour. This phenomenon could represent an association in the subjects' minds between neighboring numbers—a type of consistent missing; or, giving it a different interpretation, it could represent a general awareness of the target locus (regardless of its association with an hour of the day) with the misses being of such a nature that they could more appropriately be measured in some unit of distance. Thus displacement in the form of geometric distance between a response and the target may be another manifestation of ESP. If there is a reliable tendency for calls that miss the target to have nevertheless some geometric relationship to a target point, then an efficient method of statistically determining unknown target loci could be developed by using the information contained in each call.

First, I shall present experimental evidence supporting the following hypothesis: that calls directed to a target point in space can be arranged in distinct geometric relationship by ESP. For example, this may be manifested by a piling-up of hits about the target, similar to the British experiment with Clock Cards, or it may show itself in displacement indicated by constancy of distance or direction from the target. Then, proceeding on the basis of preliminary experiments which supported this hypothesis, I shall present a method of statistically approximating the locus of an unknown target, a method which was developed in part from some of the methods mentioned in Part I of this paper.

The experiments about to be described were carried out during the summer of 1954 as an outgrowth of an interest in dowsing. The target was a penny which was concealed under one of 25 squares arranged in five rows and five columns; that is, in a five-by-five matrix. In Series 1 the squares in the matrix were all covered by thick paper folded once for greater opaqueness. These papers were about four inches square so that a penny was well covered, though I do not pretend that sensory cues were impossible. In the subsequent two series the squares were of 25 opaque plastic tiles with recessed backs which concealed the target penny without being tilted. The target square within the five-by-five matrix was determined always by use of a table of random numbers. While the

subject was out of the room, the experimenter selected the target square, concealed the penny there, and at the same time touched and slightly moved most or all of the remaining squares so that a discerning subject could not tell the target by noting the one square that had been moved. For the first two series the squares were located on the floor or on a table; for the final, third, series they were on a card table and were placed approximately one-quarter of an inch apart. When the subject returned to the room to make his call, there was no conversation until the call was made and recorded; then the subject was usually shown where the penny really was and the target locus was recorded by the experimenter. While the subject was making his call, the experimenter looked away from the squares to minimize sensory cues, or went into an adjoining room during the selection.

To simulate a dowsing situation a motor automatism was used by the subject to make his call. Miss Sarah Springer, one of the subjects, suggested the use of a pendulum, which was made out of a heavy antique button suspended from a short length of string. The experimenter suggested to each subject that he stand at one side of the matrix and "will" the pendulum to swing toward the row containing the target penny. The same process was repeated on an adjacent side of the matrix to determine which column the penny was in, and thus two "fixes" were made to find out which of the 25 squares concealed the penny. I had the opportunity of introducing about a dozen and a half people to the use of the pendulum and found that most had no trouble getting it to move automatically, although it did not always tell the truth with equal facility.

For the greater part of the tests the writer served as experimenter, but when the experimenter took a turn as subject, his wife became the experimenter. Throughout all the tests an easy informality was the rule, though at no time was precision in method sacrificed.

Since the experimental procedure consumed so much time, only 16 to 20 calls were made by a subject during a session in Series 1. In Series 2 sessions were of standard length, 16 calls, except where otherwise noted. In all there were three series.

Series 1 comprised 138 calls. The stopping point was purely arbitrary and a halt was called because I was more motivated to try

to analyze data than to collect more. The outlook for good results seemed poor since the data contained only three direct hits (with mean chance expectation 5.5); and I was anxious to see if there were, nevertheless, some secondary effects present in the results in the form of "near misses" or partial hits.

The evaluation of displacement measured in terms of distance between the call and the target squares can be solved in several ways but the following method was finally chosen for its relative simplicity.[1] If the side of one square in the five-by-five matrix is considered as one unit of length, and distances between any two squares are measured from the two centers, then we obtain the various distance values shown in Table 1. C_1 to C_6 represent the six types of call positions possible in a five-by-five matrix, and distances are measured from each of these points to the random target point. Thus for every call made, a distance value can be found by consulting Table 1 and reading off the distance in units between the call and the target point. By this method it is possible to assess the distance factor directly.

Given a number of calls, one can determine the theoretical mean and the variance by the formulae given with Table 1, and hence their combined significance.

Since this is a new method of evaluation, I decided to check its results empirically by a control. To construct the control series, calls which had been made were matched to targets for which they were not intended but which occurred in the same session. This was accomplished by regularly comparing the calls in the first half of a session and the targets occurring in the latter half of a session, and vice versa.

By this method of analysis the total experimental data in Series 1 were found to be very close to chance, but a routine check for chronological decline within the session revealed a suggestive decline pattern in which most of the above-chance performance was confined to the first half of the session. The control data, on the other hand, showed nothing significant. This information is contained in Table 2.

[1] For the evolution and refinement of the method of evaluation I am indebted to the efforts of Mr. Malcolm Turner and Drs. J. A. Greenwood and T.N.E. Greville. For the derivation of the formulae in Table 1, I wish to thank Dr. Greenwood.

Table 1

METHOD OF DISTANCE EVALUATION

C_1	a	c	f	i	a	C_2	a	c	f	c	a	C_3	a	c
a	b	d	g	j	b	a	b	d	g	d	b	a	b	d
c	d	e	h	l	d	c	d	e	h	e	d	c	d	e
f	g	h	k	m	g	f	g	h	k	h	g	f	g	h
i	j	l	m	n	j	i	j	l	m	l	j	i	j	l
b	a	b	d	g	d	b	a	b	d	e	d	c	d	e
a	C_4	a	c	f	c	a	C_5	a	c	d	b	a	b	d
b	a	b	d	g	d	b	a	b	d	c	a	C_6	a	c
d	c	d	e	h	e	d	c	d	e	d	b	a	b	d
g	f	g	h	k	h	g	f	g	h	e	d	c	d	e

Distance Values

C_{1-6}	=0.0000	h	=3.6056
a	=1.0000	i	=4.0000
b	=1.4142	j	=4.1231
c	=2.0000	k	=4.2426
d	=2.2361	l	=4.4721
e	=2.8284	m	=5.0000
f	=3.0000	n	=5.6569
g	=3.1623		

KEY: C_{1-6}=6 basic call positions. a to n=the distances from the center of each call position to the center of calls in the matrix, using the shortest distance between centers as the unit of measure.

EVALUATION: M=Expected Mean; V=Variance; fC_{1-6}=frequency of basic call positions in an experiment.

$M = 3.1736\ fC_1 + 2.7210\ fC_2 + 2.5662\ fC_3 + 2.2213\ fC_4 + 2.0506\ fC_5 + 1.8744\ fC_6$

$V = 1.9283\ fC_1 + 1.5962\ fC_2 + 1.4146\ fC_3 + 1.0658\ fC_4 + 0.7950\ fC_5 + 0.4866\ fC_6$

$CR = \dfrac{\text{Observed sum of distance values} - M}{\sqrt{V}} \longrightarrow$ normality approaches

The decline was revealed more strikingly by the following analysis in which the data were divided into chronological halves by session, as above, and further broken down into two groups of calls: (1) those scoring hits or near misses (distance value of less than 2.0); and (2) all other calls (distance value of 2.0 and above). This means of dividing the distances into two groups was selected arbitrarily as one of convenience without regard to the results of the evaluations. From this breakdown a four-place contingency

Table 2

GENERAL RESULTS AND SESSION DECLINE ANALYSIS FOR SERIES 1

EXPERIMENTAL DATA

	Expected Total Distance	Observed Total Distance	Dev.*	CR	P
First half session.....	161.4373	142.9304	+18.5069	2.08	.038
Second half session...	167.7444	169.2273	− 1.4829	−.15	.50
Total.....	329.1817	312.1577	+17.024	1.32	.19

CR_d (1st half vs. 2nd half) = 1.55; P = .12

CONTROL DATA

First half session.....	161.4373	157.7645	+ 3.6728	.41	.50
Second half session...	167.7444	157.3305	+10.4139	1.12	.26
Total.....	329.1817	315.0950	+14.0867	1.09	.28

CR_d (1st half vs. 2nd half) = .52; P = .50

*The + sign before the deviation merely indicates that the calls tended to be nearer the target than chance expectation and vice versa.

table was made which revealed a highly significant difference between the first and second halves of the session. Table 3 gives this result and also shows that there was no such difference in the control group.

These results were so encouraging that I decided to proceed with Series 2 after adding two methodological safeguards: the plastic tiles mentioned above to minimize sensory cues, and sessions with a predetermined number of calls (16 when I was testing or being tested, with the exception that when my wife served as subject there were eight).

Series 2 was stopped when it was approximately equal in size to Series 1. The results were in the predicted direction but they can be considered only suggestive. The results appear in Tables 4 and 5, while Tables 6 and 7 summarize these first two series combined.

Although the combined results for Series 1 and 2 are only slightly above mean chance expectation, there seems to be ample evidence of a chronological decline. An additional fact we can note

Table 3

ANALYSES OF SESSION DECLINE IN SERIES 1

EXPERIMENTAL DATA

	Distances <2.0	Distances ≧2.0	Total
First half session	36	33	69
Second half session	16	53	69
Total	52	86	138

$\chi^2 = 12.34$ (1 d.f.); $P = .0005$

CONTROL DATA

	Distances <2.0	Distances ≧2.0	Total
First half session	21	48	69
Second half session	24	45	69
Total	45	93	138

$\chi^2 = .30$ (insignificant)

is that most of the decline was contributed by displacement since the number of direct target hits in each half-session is small: first half = 5 hits; second half = 3 hits. When the decline effect in the experimental data as shown in Table 7 is re-evaluated entirely on the "misses" or the trials which would reveal any ESP displacement effect, the level of significance does not change appreciably ($\chi^2 = 10.19$, 1 d.f.; $P = .0014$). Of interest is the significant above-chance performance confined to the first half of a session. This finding of a distance relationship to the target due to ESP led to the further development of an unproved statistical method for concentrating slight ESP effects for reliable use. It came about in the following way.

After Series 1 was completed, I attempted to analyze the data by a different method. This was to measure the distance on the five-by-five area from call to target in two perpendicular axes (in reality the distance the subject was off in row and column de-

Table 4

GENERAL RESULTS AND SESSION DECLINE ANALYSIS FOR SERIES 2

EXPERIMENTAL DATA

	Expected Total Distance	Observed Total Distance	Dev.	CR	P
First half session.....	141.0045	122.7222	+18.2823	2.16	.031
Second half session...	142.0542	145.6547	− 4.6005	−.54	∼.50
Total.....	283.0587	269.3769	+13.6818	1.14	.25

CR_d (1st half session vs. 2nd half) = 1.90; P = .057

CONTROL DATA

First half session.....	141.0045	144.6653	− 3.6608	.43	∼.50
Second half session...	142.1542	133.2988	+ 8.7554	1.02	.31
Total.....	283.0587	277.9641	+ 5.0946	.42	∼.50

CR_d (1st half session vs. 2nd half) = 1.03; P = .31

termination—the two fixes, or determinations, on the target locus) and perform a standard correlation operation on the pairs of values so obtained. Since then I have learned that conclusions drawn from *r's* so computed are of dubious validity inasmuch as the values of the two distances used are not normally distributed.

However, a certain amount of interesting information was found in Series 1 and confirmed in Series 2. Although the calls in the second half of the session were farther from the target locus, they were consistently closer in one axis than in the other in such a way that they gave a large negative *r*. Thus the correlation analysis measured not only distance from the target but also, as Dr. Karlis Osis first pointed out, a combination of distance and direction. After Series 2, I decided to use this approach to develop the method of concentration of ESP effect.

From a control performance on targets which could be immediately checked (index trials) I planned to obtain an index, *r*, as described above. At the same time these index calls were made, calls were to be directed against a test target point. The project was to discover whether, with a similarity of performance during

Table 5

ANALYSES OF SESSION DECLINE IN SERIES 2

EXPERIMENTAL DATA

	Distances <2.0	Distances ≧2.0	Total
First half session	22	36	58
Second half session	17	41	58
Total	39	77	116

$\chi^2 = .97$ (insignificant)

CONTROL DATA

	Distances <2.0	Distances ≧2.0	Tota
First half session	18	40	58
Second half session	19	39	58
Total	37	79	116

$\chi^2 = .04$ (insignificant)

the session, and a knowledge of the r from the index calls, it would be possible to assign probable loci to the unknown point to a reliable degree.

The following procedure was used assigning these loci in accordance with principles and conjectures already enumerated in Part I of this paper. A subject would make a number of calls carried out in a short series so as to minimize decline effects. Six calls were set as the number which would represent an adequate sample for an index of ESP and, at the same time, assure a constant performance by virtue of the chronological shortness of the series. It was decided arbitrarily that four of these six calls would be directed toward index targets and the other two would be directed against the test targets to be indicated or inferred. Whether calls were to be for index or test targets was determined by a random process based on a closed deck of cards shuffled before the start of a session. Before a call was made, the experimenter, but not the subject, knew

Table 6

TOTAL RESULTS AND SESSION DECLINE ANALYSIS IN SERIES 1 AND 2

EXPERIMENTAL DATA

	Expected Total Distance	Observed Total Distance	Dev.	CR	P
First half session.....	302.4418	265.6526	+36.7892	2.99	.0028
Second half session...	309.7986	315.8820	− 6.0834	−.48	~.50
Total.....	612.2404	581.5346	+30.7058	1.74	.082

CR_d (1st half session vs. 2nd half) = 2.43; P = .015

CONTROL DATA

First half session.....	302.4418	302.4298	+ .0120	.00	1.0
Second half session...	309.7986	290.6293	+19.1693	1.52	.13
Total.....	612.2404	593.0591	+19.1813	1.09	.28

CR_d (1st half session vs. 2nd half) = 1.09; P = .28

the class of target for which the calls were intended. As soon as a call on an index target was completed, the experimenter told the subject the result.

The problem of supplying a similar target for the trials to test the application of ESP—a target that could readily be checked and still provide a challenging problem—was met in this manner: The back yard of our home contained a level plot of grass about 80 feet square enclosed by bushes, hedges, and fences, with a scattering of trees over it. This area was divided into a five-by-five matrix containing 25 squares, each about 16 feet on a side. In one of these squares a penny was surreptitiously buried by my father-in-law, who took no further part in the experiments and, of course, told no one where the penny was. The target locus was selected from a table of random numbers. The subjects' task was to locate this penny in the back yard. On this final series the penny was hidden a total of ten different times in the back yard, making ten separate tests.

At first I proposed hiding the index targets within the back yard too, but since this would have proven inconvenient and seemed

Table 7

ANALYSES OF SESSION DECLINE IN SERIES 1 AND 2

EXPERIMENTAL DATA

	Distances <2.0	Distances ≧2.0	Total
First half session	58	69	127
Second half session	33	94	127
Total	91	163	254

$\chi^2 = 10.70$ (1 d.f.); $P = .0011$

CONTROL DATA

	Distances <2.0	Distances ≧2.0	Total
First half session	39	88	127
Second half session	43	84	127
Total	82	172	254

$\chi^2 = .29$ (insignificant)

theoretically undesirable (from the fact that there would be several pennies at once within the matrix), I decided to have the subjects dowse from a map. Accordingly, a scale map of the back yard area was made which was large enough to cover a card table. It was appropriately divided into a five-by-five matrix. Each square on the map was the size of the plastic tiles used above in Series 2 to cover the target penny. To provide a subject with an index target, a penny was placed under one of the twenty-five tiles, as with Series 2, and the map was placed over the matrix of tiles so that the penny lay under the appropriate square on the map. By this procedure sensory cues were further minimized and a readily checked set of index trials was provided. When calls were made for the back yard test target, *no* penny was under the map.

Three to eight different people took part in each test. After all available people had tried six calls, an index of performance, the r mentioned above, was computed and from this a scatter diagram of

possible target loci was made, utilizing the calls made for the test target, as described in Appendix I. Although, as noted above, the *r* is fundamentally unsound, I should emphasize at this point that any method that works is valid enough for *predictive* purposes as long as we judge the results of the prediction by valid statistical methods.

In our case, the evaluation of the results was done in this way: Before the experimenter was told the location of the back yard penny he picked its most probable location, next most likely location, etc., from the scatter diagram. In such a case, if the experimenter's first choice is correct, then the probability in that experiment is 1/25; if the experimenter's sixth choice is correct, then the probability is 6/25, etc. In case of ties, if *n* squares had an equal or larger number of marks and one of them was the target, the P of that test would be *n*/25. As an approximation, Fisher's method of combining probabilities may be used to evaluate any number of such experiments. For the first four times the penny was hidden, the *r* method of prediction was used and the results were in a positive direction, though not significantly so. The P values for these first four tasks were: 2/25, 3/25, 25/25, and 5/25 with a combined probability of approximately .18.

At this time I visited the Parapsychology Laboratory at Duke University, presented the results to date, and received a number of valuable suggestions some of which have already been mentioned in the preceding paragraphs. In addition, two important points were brought up: (1) the need for an objective way to select from the scatter diagram the probable target locus, the next most likely, etc.; (2) the necessity of clarifying and simplifying the predictive method, preferably by means other than the correlation coefficient. Since we were interested primarily in distance and possibly angle from a target, I followed a suggestion made by Mr. Malcolm Turner and created two separate indices of performance: distance and direction from the target. This new predictive method is described in detail in Appendix II.

While at Duke, I had the members of the Laboratory staff take part in a fifth test, and I evaluated the results according to this new predictive method. The result was a second place hit on the back yard penny with a P value of 2/25. This was very encouraging,

and I decided to double the series to 10 such tests. This number was conveniently reached just at the time of my induction into the Armed Forces. For these last five tests, three members of my immediate family participated as subjects. At a session each subject made 18 calls: six calls using the button pendulum previously described, six using a pair of roller-bearing dowsing rods, and six in which the subject called target squares on miniature maps of the back yard area which were prepared beforehand and concealed in envelopes.

The purpose of this change in methodology was both to add variety to the testing and to see if certain techniques favored similar performance on index and test targets. The overall results of the 10 tests carried out with the attempt at applying the results for the location of the test targets are presented in Table 8.

Table 8

GENERAL RESULTS OF INDEX TRIALS OF FINAL SERIES AND OF ATTEMPTED APPLICATION TO TEST TARGETS

Test	Index Trials	Direct Hits	Probability for Test Target Location
1	16	1	2/25
2	24	2	3/25
3	12	1	25/25
4	24	3	5/25
5	36	1	2/25
6	36	2	20/25
7	36	5	1/25
8	36	2	6/25
9	36	0	5/25
10	36	2	2/25
Total	292	19	....
	Dev. = +7.32; SD = +3.35 CR = 2.19; P = .029		Combination of probabilities by Fisher's method: P = .018

The index-test performance (that is, calls made for index targets which were immediately checked) contains evidence of ESP that is almost significant, judging from the direct hits alone. An evaluation in terms of distances from target (which includes both direct hits and displacement) was not made. The results in finding the location of the penny in the back yard are significant. Thus, the experiment which was the most vigorous in elimination of sensory

cues turned out to be the most successful in extrachance scoring for which we give ESP credit. How reliable this series would have been in actual practice can be surmised if we consider how much effort would have been saved in searching for the penny if we had to sift the ground foot by foot. On the average we would have had to sift about 12.5 squares to find the penny by chance each time; or in a series of ten, about 125 squares. With the predictive method we would accomplish the same results with an expenditure of approximately 57 per cent as much effort (in searching the 71 squares). The reader can easily visualize the manifold uses of such a system if it could be reliably employed.

Since some provision was made to separate the distance and angle factors in the last six tests, I used them separately to see which gave the better prediction. Although the differences were not statistically significant, the data indicated that almost as good predictive success results from the use of the distance measurement alone. This suggests that the angle or direction factor may turn out to be an inconstant feature of performance.

The data indicated that certain of the three methods of making calls (pendulum, dowsing rods, and direct map-reading) favored similar performance on index and test targets. These results cannot be considered conclusive but merely suggest that the technique of making the calls might be very important in the development of a reliable ESP method.

At any rate, we might summarize what we have done and learned. That much is left to be done and learned the reader can only too plainly see. Evidence of ESP has been obtained through the use of a simple motor automatism of some distance relationship between a call and the target, a locus in space. Utilizing this fact, two crude predictive systems were developed and put to use to demonstrate a possible practical application of ESP in solving a particular type of problem: the location of an unknown point in space. In 10 tests, the method of prediction gave results at a confidence level ($P = .018$) which would prove of practical importance if it could be achieved consistently and could be applied to analogous problems in everyday life.

But as mentioned earlier, before we enjoy any such practical use of ESP, we must learn to control psi to better advantage. Per-

haps we are closer than we think to controlling ESP, but only more hard work and the future will tell us that. Even a low grade of control would make the practical use of ESP a reality and open up almost incomprehensible possibilities.

Appendix I

From the index calls of each participant a measure of performance, the *r* or correlation coefficient, was computed (by doing a standard product moment correlation on the row and column distances between a call and its target). Knowing where in the five-by-five matrix the subject made his call aimed at the test target, I could select the loci most likely to be the test target by picking those squares which were related to the call in such a way as to give an *r* similar in sign to the index *r* based on the calls for the index targets. For example, if a subject's index calls gave a positive *r* and his call for the test target was in the corner of the five-by-five matrix (*C* in Fig. 1), then those squares marked "x" would represent most likely loci of the test target since these are the squares which are approximately equidistant in row and column distance from the call, *C*, and hence would give a positive *r*.

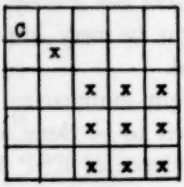

Fig. 1

By repeating the process demonstrated in Fig. 1 with each call directed against the test target, it was possible to construct a scatter diagram showing the possible loci of the test target. The number of x's in each square, when the inferences from all the index-trial correlations were pooled, indicated the likelihood that that square would contain the target. Unfortunately interpreting these diagrams required some subjective judgment, and this cumbersome method of prediction was supplanted after the first four tests by the one described in Appendix II.

Appendix II

For the r index of performance, two separate indices of performance were substituted: (1) distances and (2) angle or direction from the target. A distance index was achieved by using a set of distance values similar to those shown in Table 1 and assigning these values to the index calls and obtaining their average. From this average distance value a diagram showing possible loci of the test target could be made. For example, if the distance value was 1 and the call made for the test target was in the center of the five-by-five matrix (C in Fig. 2), then those squares approximately 1 distance unit away would be the most likely loci of the remote target ("x's" in Fig. 2).

	x	x	x	
	x	C	x	
	x	x	x	

Fig. 2

6	9	9	3	2
3	9	8	7	1
4	9	6	9	4
3	7	5	10	5
3	5	6	5	3

Fig. 3

By repeating this process with each call directed against the back yard target in each test, a scatter diagram was built up showing those squares which would probably contain the target; i.e., those squares with the largest number of "x's" in them. Fig. 3 represents such a scatter diagram for the fifth test. The number in each square indicates the number of "x's" in each.

2	4	2	5	4
4	3	3	3	5
4	1	3	5	5
3	2	3	5	2
2	4	5	4	6

Fig. 4

8	13	11	8	6
7	12	11	10	6
8	10	9	14	9
6	9	8	15	7
5	9	11	9	9

Fig. 5

To measure direction from the target proved more troublesome since the concept of direction of necessity involved some distance considerations. As developed here, the direction index reflected distance to a considerable extent. In brief, the index consisted in

arbitrarily ranking the various directions that a call in a five-by-five matrix could be from the target locus. The nearer the call was to the row or column containing the target, the lower its rank order; or put in a more directional manner, the closer a call to a north, east, south, or west direction from the target, the lower the rank order. To these rank orders, numerical values were assigned so that an average index of performance could be obtained from the index calls. From this index and the calls directed against the test target a scatter diagram showing its probable loci was made. Again, as an example, Fig. 4 shows the diagram made from the fifth test.

To select the probable target locus from these diagrams, an objective method was developed. The diagrams for distance and direction were directly combined, as shown in Fig. 5, and the square with the maximum number of marks (x's) was first choice for the test target locus; the square with the next greatest number, second choice, etc. As it turned out, the target was the second choice; hence the probability of this test was 2/25.

The essential point to remember about this predictive method is that, while it contains many arbitrary features, it was tailored to take advantage of observed characteristics of ESP performance. The method was not designed to prove these characteristics but merely to take advantage of them to *predict* the location of remote targets. Their value will be judged solely by their success at the latter task.

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WHAT THE YOUNGER PSYCHOLOGISTS THINK ABOUT ESP

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ABSTRACT: Two more polls have recently been taken of the reaction of psychologists to the research in ESP. In the following paper Dr. Warner, who was either jointly or wholly responsible for two earlier surveys made in 1938 and 1952, reports the results of the two later surveys, and summarizes the results of all four. The third and fourth surveys were made on the younger members of the American Psychological Association (the Associates admitted in 1950 and 1955, respectively) with a view to comparing the results with those of the survey made in 1952 on the Fellows of the Association. The comparison shows a rather large difference between the older and younger psychologists, the younger members showing a greater readiness to accept ESP as an established fact or likely possibility. There is some increase in the percentage who recognize the ESP research as a legitimate scientific undertaking and a much larger increase in the percentage who consider that it falls within the province of academic psychology. When compared with the figures taken from the survey made in 1938 on a census of members of the A.P.A., the results of the present poll indicate a quite impressive increase in the percentages favorably disposed toward ESP and its investigation.

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FOUR SURVEYS of the attitudes of members of the American Psychological Association toward ESP have been completed. The first survey (2) was made in 1938 with the cooperation of Dr. C. C. Clark of New York University. It provided a reasonable index of the degree to which experimental scientists are willing to take the two extremes of position, affirmation and denial. The later surveys involved more variables than just the passage of time. Between the first and the second (1), which was carried out in 1952, the Association grew from 603 Full Members to 1575 Fellows and Life Members, who were essentially the equivalent of the earlier Full Members. This increase alone, tapping further sources of human supply, conditions the comparison of the data. Beyond this, the composition of the Association had altered considerably during the period. Certain groups dropped in numbers proportionate to the whole—the psychophysicists, for example. Other groups increased their proportionate share of members, and in this connection industrial psy-

[1] The expense of this investigation was generously borne by the Parapsychology Laboratory of Duke University.

chologists and clinical psychologists occur to me as possible examples. The standards of selection for membership unquestionably changed, whether raised or lowered does not matter. Who could define the two terms in any case? For many reasons, therefore, it is understandable that comparisons between the first and second samples, separated as they were by fourteen years, is somewhat arbitrary and is defensible only in that each was as representative as possible of the mature membership of the American Psychological Association in the years 1938 and 1952.

The Earlier Surveys

The same basic questionnaire has been used in all the surveys. It is shown in Table 1. The first, most critical, question concerned belief about the possibility of ESP. The trend (as shown in Table 3) was indicated by a rise from 1.4 percent in 1938 to 2.6 percent in 1952 of respondents feeling that ESP is an established fact. A more significant increase, because many more respondents were involved, was shown by the group which considered ESP as a likely possibility. It rose from 7.4 percent to 14.0 percent. Those accepting ESP as a remote possibility also increased, though slightly, from 36.4 percent to 39.0 percent.

These shifts were at the expense of those whose opinion was indicated by the claim that ESP was an impossibility, presumably on a priori grounds. There was a small drop proportionately in the group which selected the most conservative choice: that ESP is merely an unknown.

The Recent Surveys

Recently studies have been made of two groups which we may presume to represent the mature psychologists of the future (see Tables 1 and 2). All Associates admitted to the Association in 1950 and all Associates admitted to the Association in 1955 have been interrogated by mail, using the questionnaire that was employed in the 1938 and 1952 surveys. Judging from earlier experience, the majority of these Associates will, in the course of a very few years, become Fellows, and so we may think of them as representing the Association ten or so years hence. Somewhat over 60 percent of the 621 Associates admitted in 1950 and somewhat

Table 1

RESULTS OF ASSOCIATES ADMITTED TO A.P.A. IN 1950

						Total
I. In your opinion, is "extrasensory perception"						
1. An established fact?	112	28	111	91	15	357
2. A likely possibility?						
3. A remote possibility?						
4. An impossibility?						
5. Merely an unknown?						
II.* Is this opinion based on						
1. Reports in newspapers and magazines?	38	6	42	29	8	123
2. J. B. Rhine's book?	29	6	45	30	5	115
3. Experimentation as reported chiefly in the *Journal of Parapsychology*?	31	5	28	36	5	105
4. Hearsay	18	4	14	8	1	45
5. Purely a priori grounds?	32	12	41	20	3	108
6. Other						
My scientific background	5	2	4	2	0	13
Professional meetings	1	0	2	3	0	6
Discussion with colleagues	6	0	7	1	0	14
Reports in sources other than *Journal of Parapsychology*	20	0	22	10	3	55
Discussion by Rhine	1	0	2	3	1	7
Personal experience	3	1	1	7	0	12
Personal experimentation	6	4	6	5	2	23
Not logical	0	1	0	0	0	1
Dunninger's work	0	0	1	1	0	2
Training and study in psychology	1	0	2	1	0	4
Graduate seminars	2	0	0	0	0	2
Experimentation as student	1	1	2	2	0	6
Lectures	0	0	1	0	0	1
Radio and/or TV	0	0	0	0	1	1
III. a. Do you consider the investigation of ESP a legitimate scientific undertaking?						
Yes	111	16	106	91	15	339
No	0	12	5	0	0	17
Blank	1	0	0	0	0	1
b. If so, does it fall within the province of academic psychology? Yes	101	14	98	85	14	312
Partly	1	0	3	0	0	4
No	4	6	4	2	1	17
Blank	6	8	6	4	0	24
IV. In your opinion, is the present experimental movement (as exemplified by the better controlled work reported in the *Journal of Parapsychology*) in the proper direction? Yes	29	4	33	50	8	124
Probably	0	2	8	4	0	14
Partially	11	2	3	4	0	20
No	12	8	8	2	2	32
Probably not	1	0	0	1	0	2
Blank and doubtful	32	8	28	18	3	89
Have not read it	27	4	31	12	2	76

*The base in this instance is total replies, not total respondents, since it is a multiple answer question.

Table 2

RESULTS OF ASSOCIATES ADMITTED TO A.P.A. IN 1955

							Total
I. In your opinion, is "extrasensory perception"							
1. An established fact?						31	774
2. A likely possibility?					215		
3. A remote possibility?				248			
4. An impossibility?			50				
5. Merely an unknown?	230						
II.* Is this opinion based on							
1. Reports in newspapers and magazines?	83	12	89	69	7	260	
2. J. B. Rhine's book?	63	5	61	66	8	203	
3. Experimentation as reported chiefly in the *Journal of Parapsychology*?	64	10	43	76	9	202	
4. Hearsay?	37	9	36	24	3	109	
5. Purely a priori grounds?	64	37	90	46	7	244	
6. Other							
My scientific background	6	3	8	4	1	22	
Professional meetings	5	0	7	2	0	14	
Discussion with colleagues	15	0	9	10	5	39	
Reports in sources other than *Journal of Parapsychology*	47	4	34	24	6	115	
Discussion by Rhine	4	1	3	1	1	10	
Personal experience	1	0	6	13	4	24	
Personal experimentation	9	2	5	12	3	31	
Dunninger	1	0	2	0	1	4	
Radio and/or TV	0	0	0	1	0	1	
Unexplained experiences of others	0	0	2	1	0	3	
Training and study in psychology	7	1	6	7	1	22	
Graduate seminars	3	0	0	0	0	3	
Experiments as students	2	0	1	1	1	5	
Lectures	4	0	4	5	0	13	
III. a. Do you consider the investigation of ESP a legitimate scientific undertaking?							
Yes	218	21	238	213	31	721	
No	11	28	10	2	0	51	
Blank	1	1	0	0	0	2	
b. If so, does it fall within the province of academic psychology? Yes	199	16	211	202	28	656	
Partly	3	2	1	1	0	7	
No	14	11	25	7	1	58	
Blank	14	21	11	5	2	53	
IV. In your opinion, is the present experimental movement (as exemplified by the better controlled work reported in the *Journal of Parapsychology*) in the proper direction? Yes	62	3	76	113	19	273	
Probably	13	1	5	4	5	28	
Partially	10	2	13	14	1	40	
No	27	13	20	7	1	68	
Probably not	3	1	2	1	0	7	
Blank and doubtful	76	14	73	36	2	201	
Have not read it	39	16	59	40	3	157	

*The base in this instance is total replies, not total respondents, since it is a multiple answer question.

over 60 percent of the 1323 Associates admitted in 1955 filled in and returned replies. A few more are still drifting in, but for the purposes of this article, we closed the polls December 22, 1955. If late repliers alter the picture, we shall, of course, so report. The mailing went out on September 23, 1955, which presumably would offer plenty of time. However, it is quite possible that the younger psychologists are subject to more changes of address than are the Fellows, 74 percent of whom replied in the 1952 survey. The percentage of undelivered questionnaires returned to the writer suggests this, and it appears likely, therefore, that the drop in filled-out and returned questionnaires does not indicate a drop in interest.

As has been made clear by volunteered comments from respondents in all four surveys, there are flaws in the questionnaire. There are no flawless questionnaires. When direct comparisons of the specific elements of one study with another are of primary concern, the later questionnaires should benefit through the mistakes of the earlier. In the ESP studies, however, comparisons of general trend were the main interest. Thus all flaws, as well as possible virtues, have been retained. Any trends observed in the results of the four surveys must be laid to something other than variation in the questionnaire. As a matter of fact, the flaws themselves possibly have stimulated objections from respondents which reflect trends.

Question I. The Critical Question

If we care to look at the respondents of the two latest surveys as representing the American Psychological Association of the future, we can see that the trend toward the ultimate acceptance of ESP is unmistakable. Those who now accept ESP as a fact, combined with those who regard it as a likely possibility, total 29.7 percent and 31.8 percent, respectively for the 1950 and 1955 admissions of the present survey, as compared with 8.8 percent and 16.6 percent for the surveys of 1938 and 1952 (see Table 3). This gives for the survey of today about 3.5 times as large a percentage in a much larger profession than did the poll of 17 years ago.

There remains a considerable group, almost a third of the total, who cautiously consider the entire realm of ESP to lie in the unknown. It is satisfying to see that such a large group of experimentalists still insist upon labeling a trait, which has not to their

Table 3

COMPARISON OF RESULTS OF FOUR SURVEYS

		PERCENTAGE OF RESPONDENTS			
		Full Members 1938	Fellows and Life Members 1952	Associates Admitted in 1950	Associates Admitted in 1955
Question I	1	1.4	2.6	4.2	4.0
	2	7.4	14.0	25.5	27.8
	3	36.4	39.0	31.1	32.0
	4	14.5	10.3	7.8	6.5
	5	40.3	34.1	31.4	29.7
		100.0	100.0	100.0	100.0
Question II*	1	14.7	12.7	19.1	19.7
	2	23.8	25.8	17.9	15.3
	3	21.0	18.6	16.3	15.3
	4	4.1	4.3	7.0	8.2
	5	17.4	16.9	16.8	18.4
	6 (Other)	19.0	21.7	22.9	23.1
		100.0	100.0	100.0	100.0
Question III (a)	Yes	89.2	88.8	94.9	93.1
	No	9.7	9.2	4.8	6.6
	Blank	1.1	2.0	.3	.3
		100.0	100.0	100.0	100.0
(b)	Yes	76.1	77.9	87.4	84.8
	Partly	1.7	2.9	1.1	.9
	No	7.4	10.9	4.8	7.5
	Blank	14.8	8.3	6.7	6.8
		100.0	100.0	100.0	100.0
Question IV	Yes	32.4	30.7	34.7	35.2
	Probably	2.0	1.7	3.9	3.6
	Partially	4.8	4.3	5.6	5.2
	No	13.4	17.8	9.0	8.8
	Probably not	.6	3.7	.6	.9
	Blank and doubtful	34.6	14.0	24.9	26.0
	Have not read it	12.2	27.8	21.3	20.3
		100.0	100.0	100.0	100.0

*The base in this instance is total replies, not total respondents, since it is a multiple answer question.

knowledge from personal experience or the expressed evidence of others been established, as belonging in that tremendous territory, the unknown. Is not this attitude more stimulating to experimental research than that of those who deny purely on a priori grounds? Some of the experimenters of the future may conceivably be drawn from this reservoir of truly non-committal scientists.

It is equally interesting to see the sharp drop in the proportion of respondents who categorically declare ESP to be an impossibility. Specifically, the percentages are as follows: 14.5 for the 1938 Full Members, 10.3 for the 1952 Fellows and Life members, 7.8 for the Associates admitted in 1950, 6.5 for the Associates admitted in 1955.

Regardless of the actual occurrence of ESP, we find in this trend an increased appreciation of the inadvisability of drawing conclusions from rational considerations instead of from objective data. From the strictly experimental viewpoint, impossibility is not a scientific category.

Question II. The Basis of Judgment.

Without doubt the most significant difference to be observed refers to a minority group which has grown considerably larger. In Question II (which inquires about the basis of whatever opinion was given in Question I) the first item is "Reports in Newspapers and Magazines." The percentages are 19.1 and 19.7 as compared to 14.7 and 12.7 in the earlier surveys. Perhaps our younger generation of psychologists are either reading more about ESP or are finding more material on the subject. Since the author is unaffiliated with any group disseminating information regarding ESP, he cannot say whether this is because there are more printed words on the subject or whether these printed words are more sound in their portrayal of the possibility of ESP.

A volunteered response, "Professional Meetings," shows a drop. So does the volunteered item, "Discussion with Colleagues." Another volunteered item, "Reports in Sources Other than *Journal of Parapsychology,*" shows an increase, suggesting that this journal is no longer one of only a very few sources of information.

Question III. Legitimacy of the Investigation.

One shift may be more significant than the percentage points suggest. The "Yes" answers to Question III as to whether the investigation of ESP is a legitimate undertaking have increased. Does this mean that our younger psychologists refuse to have anything thrown out of the laboratory? If so, let us applaud.

Question IV. Is the Research in the Right Direction?

At the time the original questionnaire was written, the *Journal of Parapsychology* was not only youthful but also, except for the publications of the British and American Societies of Psychical Research, was almost the only nonpopular publication which would admit articles dealing in this realm. The younger generation seems to have somewhat more confidence in the *Journal* than did (and does) the older. The proportion of respondents who failed to reply or were doubtful or who frankly said they had not read the *Journal* has remained quite constant through the years.

In the questionnaire a subquestion was inserted chiefly to stimulate suggestions on the part of those who do not consider the "present experimental movement (as exemplified by the better controlled work reported in the *Journal of Parapsychology*) to be in the proper direction." The question asked was: "If not, what criticisms have you, or what methods to suggest?" Because of the wealth of suggestions made in the recent surveys, I am reserving, for a future article, comments thereupon and also discussion of further unsolicited comments regarding ESP.

Conclusion

My interest in the views of my fellow-scientists had previously been concerned with their ways of reaching decisions regarding new claims and the supporting evidence. The attitudes shown toward the question of ESP, taken against the background of acquaintance with the evidence, should be illuminating to students both in and out of parapsychology. Perhaps as we become more familiar with the basis of knowledge and the kind of evidence generally used by scientists in making up their minds, we will be less dependent than we are on such categorical judgments as "scientific recognition," "general acceptance," and "majority opinion."

1. Warner, L. A second survey of psychological opinion on ESP. *J. Parapsychol.*, 1952, **16**, 284-95.
2. Warner, L., and Clark, C. C. A survey of psychological opinion on ESP. *J. Parapsychol.*, 1938, **2**, 296-307.

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THE CONTROVERSY IN *SCIENCE* OVER ESP

EDITORIAL: THE CRITICAL BACKGROUND

There has been a recent flare-up of criticism of the research in parapsychology. It is a comparatively long time since this has happened, and because this attack has certain significant features, it seems worth while to give some attention to it in the *Journal.* The critique appeared in the August 26, 1955, issue of *Science* in an article entitled "Science and the Supernatural" by Dr. George R. Price, a chemist in the Medical School of the University of Minnesota. This was not only the lead article in the official news-weekly of the American Association for the Advancement of Science, but it was also given what may well have been unprecedented space —nine full pages of the enlarged format of the periodical. Because of this exceptional prominence and the extreme implications of the article, it has received wider publicity than any other paper on ESP, pro or con, has ever before been given. A review of it appeared, for example, on the front page of the *New York Times* and a number of other daily and weekly periodicals in the country and abroad gave it coverage, as did also the Associated Press.

We are presenting abstracts of the Price article and of the replies to it which were published in the January 6, 1956, issue of *Science.* We were able, through the courtesy of the acting editor of *Science,* to get from the writers some of the replies not published therein; and these, with several other contributions made directly to the *Journal,* follow after the abstracts and provide a partial round-up of the reactions the Price article has brought forth. Also, since it is likely that anyone reading this recent disputation would want to view it against the background of the whole extended controversy over ESP to which it belongs, we offer first a brief outline of past criticisms of the experimental findings of parapsychology.

Prior to the mid-thirties there was no great amount of published criticism of the experimental work on ESP. Hansen and Lehman offered the counterhypothesis of involuntary whispering to explain the experimental results of the early telepathy experiments carried out by members of the S.P.R. of London. William James and

Henry Sidgwick made reply and some concession was made by Lehman. There was controversy within the psychical research circles themselves over certain of the subjects participating in the tests who were either caught at, or were suspected of, deception. Several American psychologists (for example, Titchener and Jastrow) were outspokenly critical of the whole field, but in the main the dispute over psychical research of that period had to do with the claims of mediumship (as evidence of spirit agency). The fact is, the occasional report of experiments in ESP prior to 1934 did not constitute a sufficiently strong challenge to the scientific world to stir up an active critical reaction.

The first report of the investigation of ESP at Duke University, however, did precipitate a very vigorous controversy. During the three-year period that followed, over a dozen critical articles appeared in American psychological periodicals on the subject of ESP. The aspect of research most directly specified in these attacks was the statistical treatment of the score data. When, however, the now well-known release by the President of the American Institute of Mathematical Statistics appeared in December, 1937 (in defense of the mathematics used in the evaluation of results), it was the beginning of the end on this side of the Atlantic of any important controversy over the statistics of ESP.

But if 1937 marked the end of the main strife over statistical criticism, it still left the field under fire on other points. In fact, 1938 and 1939 were turbulent years for parapsychologists, but the symposium on ESP which the American Psychological Association held at its annual meeting in Columbus in 1938 focused the criticism on the question of the experimental methods, especially on methods of recording and reporting. The answers to the questions raised were such as to silence with hardly an exception the numerous and vigorous critics of the day.

Then, following World War II, came a period of "higher" criticism. In 1949 issues of general and mathematical logic were taken up at the annual meeting of the Society for the Philosophy of Science. In 1952 the American Philosophical Society held a meeting, with no parapsychologists participating, at which the research on ESP was criticized in a framework of general methodology and the history of science; while in the same general period several ar-

ticles appeared that took exception to the philosophical interpretation which parapsychologists put on the psi results.

Now, in 1955, the issue has been brought finally to the larger court of American science in general. From the most advanced position from which scientific criticism in this country can be leveled and in the periodical which should give it widest circulation, Dr. Price has made sweeping charges (by implication) of fraud, mental abnormality, or incompetence against the research personnel in parapsychology. This implied reflection on the character and competence of the investigators themselves would seem to be a last resort which critics sometimes use when all other criticisms have failed.

If we consider, then, the course of development over which the criticism of the psi researches has run, beginning with the more pertinent technical questions of method and thinning out eventually to less tangible uncertainties of personal reference, we may reasonably look for an early ending of this already over-extended controversy over psi. This interpretation would seem to be supported by the results of the Warner poll reported in this issue, one that incidentally was taken since the appearance of the Price article in *Science*. Parapsychology evidently is gaining ground where it ought to count most heavily.

J. B. R.

ABSTRACTS OF ARTICLES IN *SCIENCE*

"Science and the Supernatural"

By George R. Price[1]

Believers in psychic phenomena appear, in the author's judgment, to have silenced opposition as the result of an impressive amount of careful experimentation. The best of the card-guessing experiments of Rhine and Soal show enormous odds against chance occurrence, while possibility of sensory cues is often eliminated by placing cards and percipient in separate buildings, far apart. Dozens of experimenters have obtained positive results in ESP experiments and the mathematical procedures have been approved by leading

[1] *Science*, August 26, 1955.

mathematicians. Almost the only defense remaining to the skeptic is ignorance regarding the work.

Moreover the ESP studies are, if valid, of enormous importance; they challenge our very concepts of space and time. And they are, Price believes, of potentially great practical importance, in spite of the fact that the results are on a very limited scale. The capacity should be a most convenient means for transmitting information from an espionage agent; it needs only the supplementary aid of information theory to utilize its fragmentary communication. ESP could even be used for such purposes as accurate forecasting of major catastrophes.

The author, however, although once convinced by the evidence of ESP, changed his mind after reading Hume's discussion of miracles. In dealing with miracles, Hume (and others quoted) preferred a theory of folly and knavery to one that violated the laws of nature. Classifying ESP with miracles, Price then offers his opinion that, while many of the findings of the ESP researches are attributable to errors and sensory cues, all the extrachance results not so explicable are dependent on deliberate fraud or mildly abnormal mental conditions. He does not claim there is evidence of this, but he tries to show that ESP is incompatible with current scientific theory—that it would be a violation of nature. Citing Broad, he shows that ESP is incompatible with the "basic limiting principles" of physics and is therefore classifiable as miraculous, supernatural, or a magical phenomenon. Not only does ESP challenge current physical theory; there is also no conceivable explanation as to how it operates or could operate—for example, precognition.

This situation, the author believes, allows only the alternatives of fraud and error; and since some of the ESP work can be accounted for by no conceivable combination of error, the theory of fraud remains as the only possible explanation. In the border zone of mediumistic practices broadly related to the experiments under discussion, fraudulent activities have often been exposed. The possibility exists that there are a few people, even professors, among the world's two billion, with the desire and the ability artfully to produce false evidence for the supernatural.

The work of S. G. Soal has been found most convincing to eminent men of great intelligence. While there is no evidence that

Soal cheated, Price says, there are ways in which he *could* have done so if he had wanted to, not only in the Soal and Goldney work with Shackleton, but also in the Soal and Bateman tests with Mrs. Stewart. Price describes how he would operate to produce by fraud the results reported by the experimenters; he would seek a few collaborators, persons with good memories, and individuals who could be made to believe the deception justifiable in a worthy cause; namely, to strengthen religious belief. A total of seven or eight of these collaborators would be needed for the two series.

He gives different methods by which, with collaborators in places of advantage, possible trickery could have been used to produce the results of the Soal tests. In several, the agent or sender, as well as the percipient, was assumed to be a collaborator. In other suggested procedures there would have been need for collaboration by the experimenter who would have had to work with the agent, but not by the agent himself. It would have been necessary to use codes, memorize target symbols, and otherwise prepare for evading the precautionary conditions. The displacement effect (that is, hitting an adjacent symbol before or after the one intended) could have been deceptively produced when desired, as easily as the high scoring on the target. The distance test results would have required a telephone call after the test to advise the percipient which record to mail. The six samples are chosen from a larger possible range of methods by which the Soal results could have been faked.

ESP experiments have been conducted before many intelligent, "open-minded" outside observers; but what is really needed, according to Price, is a demonstration before a hostile, pig-headed and skeptical critic. The old classic objection to the presence of hostile witnesses as psychologically deterrent may be only a protective device. There should be a jury of twelve prominent men, all hostile, with only a chairman with a record of success in psi experimentation. This committee should conduct ESP experiments. First, as a test of clairvoyant ESP, they should assemble packs of cards that have been given adequate mechanical shuffling and enclosed in sealed metal containers with photomicrographs of the seals. These packs should be mailed to Duke University for use in tests with ESP subjects and, on return, opened before the full jury, the cards

fanned out by machine, and the whole procedure photographically recorded.

For a test of precognition the essential element is a target selector based on a counter of radioactive scintillation. This selector would determine which of five possible lamps would be lit, and with proper shielding a safe test procedure could be devised. There could be an agent, as in Soal's tests; and he, like the percipient, could be anywhere the experimenters wished, near or far away, in the same room as the apparatus, or at a distance. The percipients' responses would be recorded and transmitted to wherever the jury was sitting. A test proposal for psychokinesis is proposed in the same vein. Telepathy requires much more elaborate consideration of controls.

The only answer that will impress the author is an adequate experiment such as he describes. He hopes his fellow-scientists will similarly withhold belief in psi until such an answer is given.

Dr. Price is a research associate in the Department of Medicine, University of Minnesota, Minneapolis.

"On 'Science and the Supernatural'"

By S. G. Soal[2]

Such a diatribe of unsupported conjecture would not have been published by an English scientific journal. In Britain the attitude of scientific men and philosophers toward ESP is far more tolerant.

The methods of possible faking proposed by Price to account for the results obtained in the ESP experiments with Shackleton and Mrs. Stewart could not have been used under the conditions reported. At least three of the six methods proposed assumed the agent was a party to the deception. Mrs. Stewart was successful with fifteen of the thirty agents tried. Of these, four were lecturers on the staff of Queen Mary College of the University of London.

Price suggested also that the experimenter working with the agent may have learned the position of the five key cards by seeing, through the aperture of the screen, the reflection of the cards in the

[2] *Science*, January 6, 1956.

agent's glasses. It can be shown that this was impossible under the physical conditions of the experimental room. Successful experiments were conducted when press representatives, among other competent observers, were present throughout, one of them even serving as agent. Good results were produced in tests of pure telepathy in which no cards were used, no glasses were worn by the agent, and the agent was a senior lecturer at London University. Collaboration of the order required and of the kind proposed by Price is unreasonable.

The absolutely fraud-proof experiment to which Price clings is an illusory idea and would not have the enduring finality he imagines it would have. A better approach would be to discover the personality characteristics of high-scoring subjects and, through the utilization of this knowledge, to aim at better repeatability by more and more investigators.

Dr. Soal is a Research Fellow in Parapsychology at Birkbeck College, University of London, and a past President of the Society for Psychical Research in London.

"Comments on 'Science and the Supernatural'"

By J. B. Rhine[3]

The publication of Price's article with such prominence as to place and space has been a net gain for parapsychology. It introduced ESP to a wide circle of very select readers. Further, the high importance, both practical and theoretical, which Price attaches to the question of ESP is of all the greater value for being the concession of a critic. Even better still is his effective demolition of all the other criticisms of ESP, restricting his own fire to the question of fraud. Finally, his article focuses the real challenge of psi to the modern world, the collision between parapsychology and the physicalistic theory of man. This is the real sting of the ESP findings; *Science* has helped make it known.

Contrary to the way of science, however, when facts conflict with theory, Price rejects the facts themselves. He denounces them as supernaturalism because they contradict mechanism, and he does

[3] *Ibid.*

so in spite of the fact that the evidence in support of them is now extraordinarily large and extensive and has been repeatedly verified under conditions of exceptionally elaborate control. It has even stood up under long and vigorous critical attack and has finally gained a respectable foothold in a number of the established university centers of the world. On the other hand, the mechanistic philosophy which opposes it has no scientific foundation whatsoever. Yet, for all its scientific refutation of mechanism, psi is nonetheless a natural phenomenon.

Price's charge of fraud derives, not from evidence but, as he indicates, from an intellectual conflict. As a theory of the ESP results, it will not appear realistic to men of science. In actual fact the experiments he challenges parapsychologists to conduct are equally unrealistic. First, his own logic condemns the experiment to futility in advance; in ridicule he classifies parapsychology as magic, ESP as a miracle, and the study as a resort to supernaturalism. How, then, would an experiment of any kind overcome that basic difficulty? Why even try one? Actually, too, the experimental design is, for the present stage and claims of parapsychology, fantastic.

The Price article illustrates the need for a good forum in American science, one to which new findings may be presented for a fair hearing instead of waiting to be dragged before the tribunal of science as something offensive and reprehensible. It might take some of the current lag out of American science if it were made unnecessary for the explorer to spend a lifetime in defense of each radically new discovery. It could even make a difference to the survival of a culture if its inquirers could be encouraged with the hope of a prompt and proper hearing for their findings.

"Compatibility of Science and ESP"

By P. E. Meehl and M. Scriven[4]

Two of those whom Price acknowledges as having read his article in manuscript wish to clarify their position. Price bases his argument against ESP on the assumptions: (1) that ESP is in-

[4] *Ibid.*

compatible with science; and (2) that modern science is already complete and correct as it stands. If either one of these assumptions is wrong, Price's argument collapses, and, as a matter of fact, *both* assumptions are untenable. History has already compiled a sorry list of names of those who have similarly insisted on the impossibility of new and valid discoveries. Actually, Price cannot even claim that any specifiable laws of physics are violated by the parapsychological findings. His superficial consideration of this aspect of psi phenomena really deals only with certain philosophical characteristics of physical laws that are said to be absent from those governing the new phenomena.

Price's "ideal experiments" are Rube Goldberg versions of the standard psi tests, plus a skeptical jury. The mechanical contrivances would be welcome if only parapsychologists could afford them; and the jury is obviously superfluous, since, according to Price's own text, we should rather believe they lie than that the experiment succeeded. The allegations of fraud are as pointless here as when made of Freud or Galileo. They are irresponsible because they were made, with all their unpleasantness, without even a pretense of evidence.

Dr. Meehl is chairman of the department of Psychology at the University of Minnesota. Mr. Scriven is a Research Fellow in the Minnesota Center for the Philosophy of Science.

"Probability, Logic, and ESP"

By P. W. Bridgman[5]

It is easier for the author to think his understanding of probability is faulty than to believe in the genuineness of ESP. While willing to take the word of the many technically competent persons involved in the grinding of the machinery by which the evaluated figures were obtained, he is in doubt concerning the logic of the application of the probability concepts to concrete events.

The only justification for drawing a conclusion from extrachance data is to be found in some sort of pertinent regularity and this has not been demonstrated. ESP, with its statement of nonchance, but

[5] *Ibid.*

with its utter failure to exhibit any regularities or to perform a single repeatable experiment, is the only instance known to the author in which a serious claim has been made that nonchance should be capitalized simply because it is nonchance.

Dr. Bridgman is Emeritus Professor of Physics at Harvard University.

"Where Is the Definitive Experiment?"

By George R. Price[6]

The only answer to his criticisms that is acceptable to the author is an adequate experiment. The most important portion of "Science and the Supernatural" was the section suggesting new experiments. The argument is that previous demonstrations of psi have not been convincing to most scientists and that it is possible to design experiments meeting all objections that the parapsychologists have made to previous suggestions for public demonstration. Neither Rhine nor Soal has criticized the test proposed in the original article as being unfair or technically faulty. Why, then, do they reject these suggestions?

To Meehl and Scriven's objection that, according to Price's own text, it would be easier to believe the whole jury of twelve lied than that ESP occurred, the answer is that in that case a still larger jury and not a smaller one would be needed. Bridgman's objection that the concept of probability is at fault is ruled out by the internal differences in the evidence; for example, when tests of telepathy and clairvoyance were alternated, Soal found evidence of telepathy but not of clairvoyance. Soal's defense of the honesty of this person or that who participated in his experiments is irrelevant. The special individuals he named need not have cheated; certain of the other methods of trickery were possible in the sessions in which these more trustworthy persons were involved. To Rhine's comment that, on the whole, the publication of the Price paper in *Science* was a good event for parapsychology, the rejoinder is made that it would be, instead, a good opportunity if the challenge of the proposed experiment were accepted.

[6] This is an abstract of Price's rejoinder (*Science*, January 6, 1956) to comments on his first article.—Ed.

"The Experiment Should Fit the Hypothesis"

By J. B. RHINE[7]

The trouble with Price's proposed experiment is that it makes an unwarranted assumption about ESP. His argument is that if ESP occurred in the experiments reported, it ought to be *counted* on to occur in the test he designed. That is not at all the way it is, and no test is any good unless it fits the hypothesis to be tested. It looks more as if Price wants to get the parapsychologists in a box, rather than to help work out an improved test procedure.

No claim has ever been made that psi works thus on demand. It might, with careful handling of the psychological factors involved, work as well in the Price test as in any other; but, again, it might not. It would be just another exploration. If that were all that could be promised by such an elaborate design, it would not be worth the cost and effort—not, at least, today. Price is presumptuously dictating terms to Nature in his proposal and incidentally telling his fellow-scientists how *they* should make up their minds about ESP.

As a matter of fact, honesty is not a new issue in science, even in parapsychology. The old method of waiting for a requisite amount of independent confirmation proportionate to the challenge of the new claim will likely still suffice, both inside and outside of the field of psi investigation.

[7] This is an abstract of Rhine's comments (*Science*, January 6, 1956) on Price's rejoinder.—Ed.

COMMUNICATIONS ON THE PRICE ARTICLE IN *SCIENCE*

The following articles and letters (alphabetically arranged) were received in consequence of Price's August 26 article in *Science*. Some, as will be apparent, were originally directed to the Acting Editor of *Science* (Dr. Dael Wolfle) who, at our request, kindly informed the authors that the editors of the *Journal of Parapsychology* were interested in having their letters. The others were submitted directly to the editor of this Journal.

To the Editor of *Science:*

George R. Price (in *Science* of August 26) states that he will not accept an experiment "on a basis of faith in human honesty." In view of the fact that all scientific experiments must be accepted —if they are to be accepted at all—on the basis of faith in the honesty of the experimenters, this scathing indictment, if regarded seriously, would destroy the entire body of science, including Price's own field of chemistry.

Price would no doubt be quick to retort that experiments in chemistry are repeatable by any skeptical chemist. But has he himself actually repeated all of the basic experiments upon which are based the tenets of modern chemistry? Or has he, like all other scientists, accepted most of these experiments on textbook statements that they have been found to be true? If he has not personally repeated each experiment which he accepts, he is admitting to a faith in human honesty for his field which he seeks to deny for parapsychology.

The distinguished American psychologist, Karl Dallenbach, has pointed out (in classes attended by the writer) that the honesty and integrity of scientists is basic to the progress of science. If scientists do not have their honesty, he has stated, what have they left?

The Price article does point up the importance of working toward a parapsychological experiment that can be repeated by any laboratory with essentially the same results, and the present writer believes that we are getting close to this goal. Then even Price, after repeating such an experiment, could be convinced—unless, of course, he suspects his own honesty in addition to that of the rest of the human race.

JAMES C. CRUMBAUGH

Dr. Crumbaugh is an instructor in the Department of Philosophy and Psychology at Memphis State College, Tennessee.

To the Editor of *Science:*

In the August 26 issue of *Science* there is a remarkable article by George R. Price; it sets limits to science that would exclude studies of extrasensory perception and it claims that all workers who obtain positive results in such studies are dishonest when not deceived.

It seems rather strange in this day and age to set limits to science. It is reminiscent of attempts to prove the impossibility of the flight of heavier-than-air craft, as a violation of natural laws. Price's argument is similar, although he is unable to find any natural law that is violated. Even if there were a violation, however, that would be no reason for exclusion, for a law of nature is merely a description of how nature has been found to behave. As new information develops, laws are constantly modified. There being no evidence of natural law violation, however, Price is reduced to pointing out that certain phenomena appear to violate "our fundamental concepts of space, time, and causality." Again he makes no attempt to *prove* incompatibility, nor does he seem to realize that these concepts, too, are subject to modification, as the theory of relativity has shown.

The only real objection that Price has to offer is the argument from ignorance: that we lack explanations of the phenomena cited. There are eight such difficulties given. First is the ability to penetrate the future without rational inference. But one of the chief aims of all science is prediction. The element of rational inference merely means we have the explanation. We do not believe that an infant who picks out a nourishing diet has a supernatural knowledge of the effects of various foods. The second difficulty is that ESP is not affected by distance. Even Price is doubtful of this criticism as he qualifies the objection by the word "apparently." As a matter of fact, it would be difficult to tell about the role of distance, whether the inverse-square law is followed or not. The third, fourth, and fifth difficulties have to do with shielding—how is it possible to pick out the design on a card in a center of a pack, for example? This would certainly require some agency or form of radiation different from ones now known. It is similar to the difficulty a blind person would have in understanding how a person could describe people in a line-up when they are all shielding each other by singing. The sixth objection is that no bodily structure is present to account for

the functions. This is similar to the argument against brain waves before Berger discovered them. This argument is of little value when so little is known about the functions of the brain. The seventh objection is that there is apt to be a decrease in ability with use, which has no parallel among other mental functions. Evidently Price is unaware of experimental extinction of conditioned responses. The last objection is that different investigators obtain different results. But that is very common in scientific investigation, due to errors and differences in method. It is a rare field in which there are no controversies.

It is clear, then, that no real objections to extrasensory perception as a field for scientific research have been advanced. Such attempts to limit science have always been unsuccessful, for the essence of scientific method is observation, and attempts to restrict observation in this field are not in the true spirit of science.

The second phase of Price's article is the contention that the positive results of Soal, Rhine, and others, being unscientific, must be due to fraud on the part of the investigators. No evidence of fraud is given, the argument being entirely a priori on the basis of the field being unscientific as discussed above. Moreover, he is careful to avoid a direct accusation of fraud, limiting himself to pointing out that Soal's results *could* have been obtained by fraud. Such an argument is difficult to meet, as it could be directed against any scientist. Normally the work of scientists is tested by agreement with other workers in the field. But Price is not satisfied by such corroboration: "Not 1000 experiments with 10 million trials and by 100 separate investigators" will satisfy him. He wants a committee of twelve—"all prominent men and all strongly hostile toward parapsychology"—to carry out the research in this field. Such a requirement is impossible, of course, for who would do research in a field toward which he was strongly hostile?

The violence of this attack on the workers in the field of ESP raises several interesting questions. The first is the problem of the acceptance of ESP. Earlier criticisms were usually based on deception of the investigator by the subject, or on the long arm of coincidence. For in many of the earlier studies, sensory clues were available, which are now carefully guarded against—often by placing the subjects tested in different rooms or buildings. When such clues

were obviously eliminated, the next objection was the statistical one—it could be a matter of chance or coincidence. By using the ESP cards, Rhine has attacked this objection with a chi-square test criterion much more rigorous than used in other scientific studies. The result has been that more and more psychologists have tended to accept the reality of ESP. Price, however, has conceived of another possibility; *all* the positively tested subjects are not only guilty of fraud but have conspired with the investigators to obtain such results. For *all* would have to be guilty, as only *one* successful experiment is required to establish the phenomenon.

As the opponents of ESP are driven to the extreme charge of wholesale fraud (on the part of both experimenters and subjects), the next question is: why this extreme "will to disbelieve"? For there are many who cannot accept ESP, but do not question the honesty of men like Rhine or Soal. When pressed, they admit they have no logical reason but yet are unable to accept the results. There are evidently both intellectual and emotional factors in such cases.

Leaving aside the emotional factors, the chief intellectual factor is probably the fact that no suitable theory has been devised that will account for the phenomena and allow a prediction of events. Even the variables involved have not been isolated. Until such problems are solved, the path of ESP will be a stormy one even without the needless accusations of fraud.

RALPH W. ERICKSON

Dr. Erickson is Chairman of the Department of Psychology, Mississippi State College for Women, Columbus, Mississippi.

To the Editor of *Science:*

George R. Price, in his article on "Science and the Supernatural" in the August 26th issue of *Science,* reminds me of the geometer in the following fanciful early nineteenth-century colloquy:

> ASTRONOMER: "I have measured, with the most painstaking care, the sum of the angles of ten thousand triangles, using heavenly bodies as apices, and have found it to be $181° \pm 0.05°$."
>
> GEOMETER: (after studying data) "I am completely convinced that your results are statistically valid and can find no technical fault in your methods, but your results, disagreeing as they so obviously do with irrefutably logical deductions from the self-evident axioms of Euclidean geometry, the Geometry of the Universe, I

> must reject. Having no plausible alternative, I am compelled to raise the question of fraud on your part, although I can suggest no sensible reason why you, a man of science, should wish to falsify your data."

Whereas the geometer has unwittingly made one false assumption, Price has made at least two:

1. *Modern science is a completed field of knowledge with apodictic laws.*

> "A miracle is a violation of the laws of nature; and as a firm and unalterable experience has established these laws . . ." . . . . . "". . . the absolute impossibility or miraculous nature of the events . . ." (as quoted by Price from Hume).

Throughout history, science has continued to discover new empirical facts and has been forced to modify its hypotheses to include them. For example, Ptolemy constructed an elaborate system of cycles and epicycles to account for the motions of the planets, based upon the geocentric hypothesis. As observations became more precise, the *ad hoc* hypotheses that would have been necessary to maintain this point of view became ever more numerous. Copernicus, returning to the ancient heliocentric hypothesis, brought order out of the growing disorder. Kepler's subsequent planetary laws, based upon further observation, remained brute facts until embraced by Newton's law of gravitation, from which they then followed logically. Eventually, however, Newton's law was found wanting to account for the rotation of Mercury's perihelion, and this discrepancy remained unresolved until unexpectedly accounted for by Einstein's modification of Newton's law.

The idea, then, that a specific way of accounting for a group of observations is provisional is certainly not new; but the idea that scientific tools and basic concepts may be similarly provisional is much less obvious, but true, for we have, in our own century, seen an outstanding example in the now unquestioned non-independence of matter and energy.

It is possible to derive an equation, such as a Fourier series, giving a curve of arbitrarily close approximation to, say, the profile of a nose drawn on graph paper. Yet, however closely the curve of the equation may fit the curve of the nose, it will not in general even approximate, when extrapolated, the profile of the whole face,

although a second equation can be derived that will approximate the whole face, but not the neck, and so forth. The form of the equation says nothing about the nature of the profile: indeed, in general, equation and profile have in common only the shapes of their curves, and then only approximately and when drawn to the same scale and between specific limits.

Scientific data may be likened to the profile; the laws of science, molded by the current mathematical-mechanistic-materialistic framework of modern scientific theory, to the equation. The form of the law (equation) depends as much upon the form of the framework (series) as upon the form of the data (profile): the color of the image perceived by my eye depends as much upon its ability to perceive color as upon the color of the object. For the positivist, the equation is simply *descriptive;* for the mechanist, it is *prescriptive.* For the positivist, a law of science describes the correlations observed among data; other things being equal, the choice of particular framework, and thereby law, is dictated by considerations of simplicity rather than necessity. For the mechanist, a law has heuristic properties, consisting in its ability to predict new facts to be observed; but even frequent, accurate prediction—i.e., heuristic success—can scarcely be turned into a proof that the law and the basic concepts upon which it rests are generally true.

Like the geometer's, Price's dogmatic belief in the profound and absolute truth of the "laws of science," though clothed in modern guise, belongs to another day, that of Haeckel and Hume. The geometer fell into the error of regarding Euclidean geometry as prescriptive because it seemed then perfectly descriptive of reality; his experience had simply not yet been sufficient for him to recognize the tacit assumption involved: Price, having the chastening experience available to him, fails to understand its significance, takes leave of the geometer, and, mechanism in hand, goes on to metaphysics.[1]

2. *Modern science is an adequate world hypothesis.*

> *"We can describe in detail the steps involved in the creation of a great poem."* (Price.)

It is startling to learn from Price that science can describe in

[1] Price thus reveals himself to be a mechanist in the metaphysical sense, and not a scientific positivist at all.

detail the steps involved in the creation of a great poem. Now I might be prepared to grant that modern science may, in principle, be capable of describing (though hardly *in detail*) the steps that occur from the appearance of the inspired thought in the poet's mind to translation of word patterns into motor impulses actuating the fingers holding the poet's pen writing conventional letter sequences bearing a one-to-one correspondence to the words chosen to express the thought. But, *where does the inspired thought come from?* From some electrochemical process that is the resultant of other electrochemical processes occurring in concatenations of engrams representing memory, associations, and what not, all triggered by an electrochemical impulse arising either internally from the poet's perception of, say, his upset stomach, or externally from some stimulus to his senses? The arrogant superficiality of this sequence is rivaled only by its obscure complexity; in fact in its essence it is more complex than any sequence of steps science can now suggest as allowing the verbalized inspiration to be recorded on paper. Yet to Price science is so well on its way to an understanding of the latter that he seems to overlook the former.

Although Price sees no difficulty in tracing, in detail, the steps from inspired thought to words on paper, nor even any difficulty in tracing the steps from, say, a telepathically perceived symbol, *once it is perceived,* to its notation on paper, he asserts that the sudden perception of the symbol by the percipient's mind is beyond the power of science to explain. It seems to me that the sudden perception of the inspired thought by the poet's mind is equally beyond the power of science to explain. Price asks whether anyone can "describe a conceivable nerve network for interpreting the raw data of ESP": I ask whether anyone can describe a conceivable nerve network for deriving an inspired thought from, say, an upset stomach. If anyone objects that we know where the symbol was before the percipient's mind perceived it—that we should be able, therefore, to outline as plausible a series of steps to account for its arrival-and-perception in the mind as we think we can when my mind perceives a tree—whereas we do not know where the thought was before it inspired the poet, I ask, granting that we are ignorant of any external source for the poet's thought, *must* we then conclude that the thought came from somewhere else in the poet's mind,

some subconscious level perhaps—that therefore we are not required to describe how it arrived there, since it was there all the time anyway? Ignorance is not knowledge, and in the current absence of any sensible, much less mechanistic, scientific explanation of how consciousness arises out of the brain, science is in no position at all to speak with presumptuously positive finality about steps involved in the appearance in consciousness of poetic inspirations. The source of the inspiration may well be, for all science now knows, as truly external to the poet's mind as the tree I perceive; nevertheless, since no one will seriously maintain that great poems do not exist (even notwithstanding those fraudulently composed!), whereas extrasensory perception, if it in fact exists, is certainly far from obvious, Price seems to adjure us, if we ask about the source of inspiration or telepathic symbol, to disregard the so-called law of insufficient reason and to assume the source to be wherever it will discommode science's current assumptions the least. Indeed, we may, even now, know more about extrasensory perception than we do about poetic inspirations: we seem to know the source of the telepathic symbol. In neither case do we know the steps in the whole process, except here and there, and then only with wretched vagueness; in both cases, in our ignorance or in fact, we seem to be dealing with something more or less akin to the miraculous—something inaccessible thus far to modern science.

* * *

Although it is undoubtedly an expedient criterion for separating wheat from chaff, the truth of any new observation cannot be judged from its compatibility with a science whose laws are only descriptions of perceived reality, and not complete descriptions at that. This is not to say that any and all observations must be accepted uncritically as being of equal validity; but if an observation survives extensive critical analysis without challenge other than on the ground of incompatibility with science—a challenge, I repeat, that cannot, without smug presumption, be used as absolute evidence against it—the observation ought to be recognized as a tentative fact, to be subjected, of course, to further scrutiny—along some of Price's lines—but also to be used, with due circumspection, in an attempt to breach knowledge barriers. Any other attitude is like that of the proverbial ostrich.

But after all, it is really of secondary concern whether Price is merely right or wrong in accusing parapsychologists of fraud, whether, therefore, extrasensory perception is someday found to be a chimera; the gross overestimation of the power of modern science and the misunderstanding of its relation to reality—those petrified, petrifying anachronisms that afflict not only Price but many others —are of primary concern as stultifying illusions that must be recognized and understood before science and reality can make a closer approach.

WALDRON R. GARDINER

Dr. Gardiner is an Instructor in Pathology at the Stanford University School of Medicine, San Francisco.

To the Editor of *Science:*

The critical issue raised in George R. Price's recent paper on "Science and the Supernatural" is not, as he states a choice between science and the miraculous but rather a question of whether or not current research methods in parapsychology are yielding significant and valid results. His analysis of the evidence contains grievous errors both of fact and method. I mention here only two of these.

The first concerns method. To impute, without evidence, deliberate fraud by responsible and scientifically trained workers in this (or any) field is neither good science nor good ethics. In this context it is especially reprehensible in view of the fact that for many decades unusual attention has been given by experimenters to the necessity of guarding against deception by the individuals under test. Consequently a high standard of vigilance has developed, with an extraordinary alertness to all possible weaknesses in the test procedures and controls. Price's belated apprehensions on the question of safeguards against fraud show an unfamiliarity with the history of the field he attacks.

My second point affords a measure of Price's responsibility as a reviewer. In his first paragraph he states: "What is unique about the present is that, during the last 15 years, scarcely a single scientific paper has appeared attacking the work of the parapsychologists." Yet he himself cites several such papers that appeared during the period in question and I have counted a dozen more without

trying to be exhaustive.[2] While it is true there has been a decline in the number of critical attacks per year, this is not at all the picture Price presents. If he is so obviously unfamiliar with the literature which supports his own polemic, the question may properly be raised whether he had any better understanding of the more extensive literature which records the positive findings in the domain of psi phenomena. There is, in fact, evidence in his paper that he does not.

EDMOND P. GIBSON

Mr. Gibson is engaged in engineering work in Grand Rapids and is President of the Michigan Archeological Society.

To the Editor of the *Journal of Parapsychology:*

In his August 26 article in *Science,* George R. Price suggests the way out for those who have been disturbed by the distasteful dilemma of parapsychological findings. "If, then, parapsychology and modern science are incompatible, why not reject parapsychology?"

Anticipating the possibility that some readers might not be too eager to jump on this bandwagon of a Dark Ages philosophy and logic, Price offers us his own personal testimonial. He, too, believed in ESP about 15 years ago, when he read scientific books based upon "an impressive amount of careful experimentation and intelligent argumentation." But, he tells us, he was saved from this empirical iniquity when he found the antiquated speculative philosophies of Hume and Lucian. With his "adamantine faith" restored, Price directs himself to the vital task of protecting the scientific public from the "knavery and folly of men."

He tells us: "Believers in psychic phenomena . . . appear to have won a decisive victory and virtually silenced opposition. . . . What is unique about the present is that, during the last 15 years, scarcely a single scientific paper has appeared attacking work of the parapsychologists." Price tells us that he is aware that explanations of ESP in terms of clerical errors, sensory cues, and fallacious statistical methods have been advanced; but he says, "I am also convinced that some of Rhine's work and most of Soal's can be accounted for by no conceivable combination of such explanations."

[2] A list of these articles has been filed with the editor of the *Journal.*

And, so, after taking the reader on an irrelevant excursion into information theory; positing an inappropriate analogy of ESP and magic, voodoo, and witchcraft; reviewing Lucian's warning that "to defend one's mind against these follies, a man must have an adamantine faith so that, even if he is not able to detect the precise trick by which the illusion is produced, he at least retains his conviction that the whole thing is a lie and an impossibility," Price cocks his scientific eye and asks, "What about fraud?" Then after some irrelevant anecdotal tid-bits about some fraudulent mediums, he is ready to take on the Soal work which he apparently considers as the strongest evidence for ESP. However, lest the reader get any wrong impressions, Price is gracious enough to say, "Before I continue, it should be clearly understood that I am not here stating that Soal or any of his associates was guilty of deliberate fraud [*sic*]. All that I want to do is show that fraud was easily possible."

Finally, the *pièce de résistance* by Price: He now takes us on a most ingenious tour of plots, schemes, and tricks with agents, observers, experimenters, and recorders collaborating in these nefarious undertakings—dupes and zombies, seduced by the wily subterfuges and promises of "The Brain" who has told them that the end justifies the means, and that their moral sacrifice will obtain much good for Humanity by strengthening religious belief. And as the curtain falls on Price's little drama, he makes a fervent plea for his fellow-scientists to withhold belief. Then, martyr to science that he is, Price warns of the slings and arrows that he can expect from the parapsychologists.

To me, too much of this sort of thing already permeates the intellectual atmosphere of our culture. Witch-hunting has seldom facilitated scientific endeavor. Unfortunately, the history of scientific development indicates that those scientists who have been bold enough to venture into the borderlands have been most frequently victimized by those who are restricted by a scientific myopia. The publication of Price's articles in *Science* has clothed a contradictory, malicious, and unscientific paper with scientific respectability. From the press releases, it would even appear that many people made the interpretation that the paper had the endorsement of the scientific profession. I do not think that many scientists would want to endorse a viewpoint that implied that if a man's work led

him to conclusions that violated the "laws of nature," he should be suspected of fraud. The history of science is a history of "breakthroughs." We should do all we can to facilitate, rather than impede, these breakthroughs.

Others besides Price have been unable to resolve the conflict between ESP and mechanism, but without resorting to uncouth personal attack upon the investigators responsible for the evidence of psi. For example, D. O. Hebb, McGill University psychologist, writes: "A final and extreme example of the present day: why do we not accept ESP as a psychological fact? Rhine has offered enough evidence to have convinced us on almost any other issue where one could make some guess as to the mechanics of the disputed process. Some of his evidence has been explained away, but as far as I can find out, not all of it. Until a complete rebuttal is provided or until we accept ESP, let us not talk about enlarging our notions of neurology to meet the psychological 'facts' with no external criterion of what those facts are. We are still trying to find our way out of the magic wood of animism, where psychology began historically, and we cannot give up the talisman of a knowledge of material processes. Personally, I do not accept ESP for a moment, because it does not make sense. My external criteria, both of physics and of physiology, say that ESP is not a fact despite the behavioral evidence that has been reported. I cannot see what other basis my colleagues have for rejecting it; and if they are using my basis, they and I are allowing psychological evidence to be passed on by physical and physiological censors. Rhine may still turn out to be right, improbable as I think that is, and my own rejection of his views is—in the literal sense—prejudice."[3]

This self-analysis is at least fairminded and discerning in contrast to the Price attack on the issue.

JACK KAPCHAN

Dr. Kapchan is Assistant Professor of Psychology at the University of Miami, Florida.

To the Editor of the *Journal of Parapsychology:*

In *Science* recently there appeared, under the title "Science and the Supernatural," a brilliant, satirical exposition of the evidential

[3] "The Role of Neurological Ideas in Psychology," *Journal of Personality*, Sept., 1951.

status of extrasensory perception. In the guise of an attack upon ESP, George R. Price presented a detailed summary of the arguments for and against both the reality and the scientific importance of psi phenomena. Some indication of the unusual nature of this article lies in the fact that it was accepted and given the leading position in the official journal of the American Association for the Advancement of Science, although it is several times the length of articles normally published by that journal.

The advancement of parapsychology has suffered from neglect by orthodox science. As Price says, "during the last 15 years, scarcely a single scientific paper has appeared attacking the work of the parapsychologists." At the conscious level this neglect is supported by two kinds of arguments: (1) that all of the evidence is defective because some statistical or experimental weakness has been found in some of the published papers; and (2) that even if real, ESP and related effects are trivially small. While pretending to mount a new and more effective attack upon ESP, Price first carefully demolishes these old arguments. His story of the man who turned into a little bat and flew away will stay in our memories a long time. He titillates the imagination by relating ESP to the mathematics of "communication theory"; and he reluctantly concedes that—taken at face value—the data of Rhine, Soal, *et al.*, make out an irrefutable case for the reality of ESP.

In order to assure emotional as well as intellectual assent to his support of psi phenomena, Price weaves in a complex, countervailing argument, designed to appeal to the prejudice and methodological ignorance of his readers. His evident purpose is to channel the unassailably diffuse opposition of scientists into a single blind alley where it can be captured and destroyed by exhibition.

To do this, he explains that ESP and precognition are rationally impossible phenomena, and belong in the class of miracles. His discussion of "magic versus mechanism" should become a standard reading reference for future students of science. With straight face and tongue-in-cheek he explains that magic is anything for which we cannot conceive a mechanistic explanation.

> "The essential characteristic of magic is that phenomena occur that can most easily be explained in terms of action by invisible intelligent beings. The essence of science is mechanism. . . . Suppose

> that some extraordinary new phenomenon is reported: should we be narrow-minded or receptive? The test is to attempt to imagine a detailed mechanistic explanation."

He leaves it to the astute reader to remember that mechanism, while it still dominates the thinking of the average scientist, was destroyed by the findings of modern physics in the first three decades of the present century. Physics today has embraced "magic," although it does so under the austere name of "logical positivism."

By denuding the skeptic of his defenses, while supporting his rational opposition to ESP, the author creates a conflict that must somehow be resolved. How does one explain away the data of Rhine and the recent spectacular success of Soal?

Easily! Soal, Rhine and all of the others are dishonest or deranged. Beginning with appropriate quotations from David Hume's chapter "Of Miracles" in *An Enquiry Concerning Human Understanding,* Price prepares the ground for his distortion of our cultural perspective. Progressing through the history of fraud, he says: "For example, the man who knows that the Davenport brothers employed as many as ten confederates in a single séance should not think it unreasonable when I presently suggest that I would want seven or eight confederates in order to imitate 170 Soal sittings."

Why might Soal do this and how would he recruit his confederates? Not by appealing "to desire for fame or material gain but to the noblest motives, arguing that much good to humanity could result from a small deception designed to strengthen religious belief." The author then explains six different procedures whereby, with a sufficient number of confederates, he could duplicate Soal's results.

As a conclusion to his article Price gives his version of a really fraud-proof experiment. His plan uses photomicroscopy of welded seals, motion picture photography of the experimental proceedings, and complex electronic circuitry. Thus, he converts the science laboratory into a court of law, cleverly forgetting that science draws its self-assurance, not from a guarantee of honesty among its practitioners, but from the independent validation of its findings.

The degree of ingenuity shown by the author in achieving his end can be fully appreciated only by someone familiar with the field. To lend credibility to his position, he quotes with evident good

humor some ill-advised remarks of modern parapsychologists and exhibits the unsavory history of early investigations into spiritualism. Repeatedly he uses the two-edged comment—seeming to attack parapsychology, yet actually conveying a factual message that parapsychologists have offered to deaf ears for many years. (For example, Footnote 54 contains information that there are more than 49 volumes in the *Proceedings of the Society for Psychical Research,* and the tacit suggestion that these are possibly worthy of the skeptic's attention.) His documentation is so extensive that it might seem to be the work of several experts. His choice of references shows a discriminating sense of humor, for example, Nabours (25) and Skinner (26). Parapsychology will be well served if these are widely read.

We have here a satirical tour-de-force that in its supposition of wide-spread fraud is without precedent in the journals of science. It will be embraced unreservedly by those who are blindly opposed to psi; it will rally to the defense of fair play and decency those scientists who have been sensitized by the political hysteria of our time; and it will be enjoyed for the rollicking good fun that it is by those few who understand scientific method and take time to make a dispassionate analysis. Above all, it may do just what its author intended. It may create a controversy where one was long overdue.

The undersigned has never met Price and, prior to the appearance of his *Science* article, had no idea what his attitude toward psi might be. The present analysis was written without consulting him, and does not necessarily reflect his position. In his ignorance of Price's real intention the writer feels at liberty to give what seems to him the most likely interpretation. If some members of the A.A.A.S. think they have been deceived, they should remember that it was not by anyone who might call himself a parapsychologist, and it was done (as far as the undersigned can determine) without the knowledge of anyone who has been publicly associated with this research.

R. A. McConnell

Dr. McConnell is Assistant Professor of Biophysics, University of Pittsburgh, Pennsylvania.

To the Editor of the *Journal of Parapsychology:*

One of the most noteworthy features of Price's treatment of parapsychology is the recognition which he gives, not only to the ability of the parapsychologists, but also to the extent to which they have won their way towards general acceptance. These encomia appear on the first page of Price's first article, in the August 26 number of *Science.* So sweeping has the victory of the parapsychologists been, Price tells us, that "during the last 15 years, scarcely a single scientific paper has appeared attacking" their work. And this success was not gained by some happy fluke, some chance stroke of good luck, he says, but as "the result of an impressive amount of careful experimentation and intelligent argumentation. The best of the card-guessing experiments of Rhine and Soal show enormous odds against chance occurrence, while possibility of sensory clues is often eliminated by placing cards and percipient in separate buildings far apart. Dozens of experimenters have obtained positive results in ESP experiments, and the mathematical procedures have been approved by leading statisticians."

Price names two books especially that have been influential in securing conviction, *Extrasensory Perception After Sixty Years* by Rhine *et al.*, and *Modern Experiments in Telepathy* by Soal and Bateman, and says that he suspects that most scientists who have read them "have found it necessary to accept their findings." These are remarkable testimonials, and coming from one who is so bitter an opponent of parapsychological findings, they are all the more impressive.

Later Price explains that he considers all the extrachance results of the parapsychologists, except for minor errors, as having been obtained by deliberate fraud.[4] This extraordinary charge of fraud brought against the leading research workers, men of high academic standing, would, I think, be very difficult to match in all the record of scientific controversy. In view of the high praise which Price gives to the parapsychologists in the passages just quoted, this charge of deliberate fraud will seem, I think, to the minds of intelligent people, distinctly improbable and incongruous. Nor will it commend itself as a desirable weapon to be used against one's opponents in future scientific controversies.

[4] Price says "deliberate fraud or mildly abnormal mental conditions," but the latter phrase is irrelevant because it does not explain extrachance results.

One of the most interesting phases of Price's thinking has to do with his views on the question of the relation of experiment to a priori judgment. The view of present-day science, I take it, is that the correctly performed experiment is final, and if the experimental results clash with previously held theory, the latter must be changed to conform with experiment. But Price's view is different. We first sit down and imagine. We may imagine any number of forces that we choose, whether they exist or not. If with such liberty, we can picture a mechanistic explanation for the assumed phenomenon, we can go ahead and experiment, and the results of the experiment will be accepted. But if we cannot imagine any possible mechanistic explanation, then the thing must be held impossible, even though experiment seems to prove it. The imagining, the a priori judgment, comes first and has the controlling power, deciding what may and what may not be allowed among supposed experimental results.

Several objections occur. What if some people challenge the mechanistic hypothesis itself? Some do. They think that there are personal, non-physical, mental or spiritual forces in the universe that cannot be reduced to mechanism. The unique advantage of parapsychology is that it is capable of showing concrete evidence of them. What shall we do in such cases? If there is freedom of experiment, we can try by experiment to find who is right.

More and more as Price proceeds in his discussion, and almost entirely in his rebuttal, he comes to lay stress supremely on a single argument. Vanished are the charges of fraud, forgotten the earlier stress on the impossibility of psi phenomena, and everything is now made to turn on the demand for the one great experiment that will settle all.

Price claims that all previous demonstrations of psi phenomena have not been convincing to most scientists because there has never been any fully fraud-proof test carried out; that he has devised such a test; that he has challenged everyone to tell him of anything that they can see that is wrong with the test; and that no one has been able to point out any serious fault in it; but that none of the parapsychologists has come forward to take it. Why? Price asks. (In reply Rhine did expose its fallacy in his final comment in *Science* of January 6.)

Now how about this great experiment, to which parapsychologists are to commit their whole case, the experiment against which no one is able to find any important criticism, the experiment the evasion of which by the leaders will bring the downfall of parapsychology? Well, let us see just what the experiment was as given by Price in its original form. It is supervised by the worst-packed committee that I have ever read or heard of. The committee that supervises the test is made up of twelve men of whom eight (two-thirds) are all *strongly hostile* toward parapsychology. But that is not all! The two-thirds who are all strongly hostile towards parapsychology are not only strongly hostile, but are strongly hostile "with that 'adamantine faith' that Lucian recommended." Here, as Price went back two hundred years for Hume's wisdom on miracles, he goes back over seventeen hundred years to the Greek satirist and humorist to find how to constitute his would-be all-powerful committee. What is the "adamantine faith" of Lucian? Price tells us that it is a negative mental fixation so rigid that even if a man saw before his own eyes the positive evidence, he would say that the whole thing was a lie and an impossibility anyway.

Dr. Price in his rebuttal seeks to obviate some of the difficulties in the way of the acceptance of his proposed test by making concessions as to the personnel of his supervisory committee such as "letting the examining committee consist of seven parapsychologists and eight skeptics since a seven-to-eight ratio would appear fairer than the one-to-two ratio I previously proposed." But he suggests no alteration in the original, that the skeptics shall be all "strongly hostile toward parapsychology with that 'adamantine faith' that Lucian recommended." We have therefore in its revised form a committee of fifteen, of whom seven are friendly to parapsychology, and eight have the adamantine negative fixation against it. Immediately after this concession, Dr. Price says, "But nobody has yet pointed out to me any important defect."!

It is an indication of how deep this sort of thinking lies in Dr. Price's mind that in another passage in his initial article he expresses the same thought in a different way. This time he does not go back to Lucian and Hume for his vocabulary, but draws his terms from his own mind. He says that parapsychologists ought to prove their case "to the most hostile, pig-headed, and skeptical of critics."

It is an interesting conception, this, that the judgment of the most hostile and pig-headed men should be adopted as our criterion of truth.

Returning to the question of our choice of tests, it is well known that the tests that are applicable to various fields of research vary with the field. The type of method to employ in research is determined by Nature herself. The wise researcher adjusts his methods to the realities of his field. There are many lines of research that cannot be concluded by a single grand experiment. When Darwin initiated the fight about evolution, the conflict over it lasted a long time as scholars slowly gathered the data bearing on the subject and year after year patiently evaluated them. Suppose Price had come along and had banged the drums and blown the trumpets and had said, "Now just throw all the data you have gathered into the wastebasket and we will settle the whole question of evolution by one grand experiment." What would people have thought of him?

Now the field of parapsychology is a subtle, variable, and difficult one. The parapsychologists did not make it so. Nature made it so. One of the clearest things about it is that psi sometimes appears and sometimes is absent. Because of this the supreme instrument of research is the statistical method. That method is well known and approved scientifically, for modern science, as all competent scientists well know, is coming more and more to deal, not with absolute, but with probable values. Of course, Price knows this very well, but he seeks above all else to prevent the parapsychologists from using the well-known instrument that is best suited to their work. And what means does he use towards this end? Not argument, but ridicule. "Not 1000 experiments with 10 million trials and by 100 separate investigators giving total odds against chance of 10^{1000} to 1—but just one good experiment." This is not argument but sheer burlesque. It raises very seriously the question: What is Price seeking? Is it the truth or is he seeking by any means and any tricks to discredit parapsychologists and to overthrow their work?

We cannot be sure of the future, but my forecast is that the present attack on the work of Rhine and Soal will be the last major attack that the parapsychologists will have to meet, as it was the most unrestrained and brutal one. I doubt whether any young man,

watching the course of Price in this controversy, will be inspired to emulate his example. And if there should be in the future an adventurer who did try, I incline to think that the future editors of *Science,* whoever they may be, will not be quite as eager as Wolfle was to throw the vast prestige of the magazine which represents the fifty thousand scientists of the A.A.A.S. behind the accusations and arguments of the newcomer.

If this forecast is correct, I think that it will mean that the scientists of America will have to regard parapsychology as something that will be permanently with us, a part of the real world of science to which they must become adjusted and which they must seek to understand. It will involve a long period of unsettling as the new world that is opening to our vision finds its place in relation to the old, but it will be a period also of supremely stimulating endeavor and large opportunity, with discoveries that will give new meaning to life.

CHARLES E. OZANNE

Mr. Ozanne, a retired Cleveland educator, originally prepared for the ministry. His twenty years of "retirement" have been spent in the active study and advancement of parapsychology.

To the Editor of *Science:*

There has recently appeared in *Science* an article by G. R. Price entitled "Science and the Supernatural." Price's paper was highly critical of those who espouse extrasensory perception, and it speculated in detail about the possibility of fraud in research on ESP.

Price expected strong objections from the parapyschological camp. He did not, perhaps, anticipate criticism from those who, like the present writer, are quite certain that ESP does *not* exist; nevertheless, his paper compels such criticism. It combines obvious misinformation and apparent malice and, in effect, lodges completely unwarranted charges of calculated dishonesty against such investigators as Soal and Rhine. The publication of such an article is most regrettable for a number of reasons.

KENDON SMITH

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EDITORIAL COMMENT: THE BALANCE OF THE VIEW

If the amount of resistance encountered by a new claim is any gauge of its importance—then the significance of the problems of parapsychology has been long and well established. If the growing vehemence of the criticism is any indication of the desperation of the opposition, the battle for recognition must indeed be nearly won. And if continued success in surviving a long and vigorous series of attacks is any measure of its growing strength, the psi research program must have very considerable vigor.

Happily there are other marks of advancement that parallel the ESP controversy already outlined. This may be a proper time to review, for example, some of the nods of approval, official and otherwise, given this much embattled branch of science. Such a review may be needed to offset the distorting effects which controversies are likely to have. It must be expected, of course, that the list will be brief; in fact, it is not a little surprising that there already are the signs of recognition we cite.

In the modern world the most general recognition that can be given to a new area of discovery is that which comes from the universities. These centers of learning, it is true, seldom by themselves initiate radical innovations, but they do eventually play an essential role in their examination and final appraisal. Either a new claim finally becomes a topic of university research itself or it drops by the wayside and is forgotten. As a matter of fact, during the first fifty years of the scientific history of parapsychology its claims had only slight success in gaining a foothold in university laboratories. During the last quarter of the nineteenth and the first of the twentieth centuries, however, certain universities in the U. S. A. did commit themselves so far as to accept funds for psychical research (e.g., Pennsylvania, Stanford, Harvard, and Clark); and in consequence there were a few more or less systematic investigations and official reports. These were important as beginnings in the university world.

At Clark University in 1926 a series of lectures was given, published under the title "The Case For and Against Psychical Belief." The volume included, among other matters, a review of the experimental evidence for telepathy then available, the best of which,

incidentally, had been produced in a university laboratory—that of Groningen. During the 1930's and 1940's a number of universities in Britain and the U.S.A. (among them Cambridge, Duke, London, Munich, and Oxford) went so far as to grant the doctorate for theses making contributions to the evidence for extrasensory perception or to its interpretations and implications. In 1953 the famous old University of Utrecht not only established a chair in parapsychology but also, jointly with the Netherlands Ministry of Education, sponsored an international conference on parapsychology.

It is a noteworthy occurrence when a new field begins to receive attention from those learned societies not immediately concerned. Among the first to take up the subject of parapsychology was the Aristotelian Society of Great Britain. In 1937, 1950, and again in 1954, the Society held symposia on one or another of the problems of parapsychology. In 1950 in the same country, the Society of Experimental Biologists conducted a symposium on the evidence for extrasensory perception. In 1950 the Royal Society of Medicine (London) sponsored a lecture on parapsychology before its Psychiatry Section, and in 1950 the famous Royal Institution of Great Britain scheduled one of its formal weekly discourses on the subject. Still another important step in recognition must be credited to that same country—the mother country of parapsychology. The Ciba Foundation of London held an international symposium on extrasensory perception in May, 1955. In the U.S.A. the one such occasion that can be listed was the symposium on ESP held by the Eastern Division of the American Philosophical Association in December, 1954.

There have been, of course, on both sides of the Atlantic other meetings scheduled on parapsychology by psychological organizations to which papers on the subject have been submitted in the usual routine. This type of participation ordinarily implies no special recognition beyond the presence of an interested or broadminded member of the program committee.

Among the most careful and conservative of social institutions is the philanthropic foundation. It is a token of progress, then, that parapsychology has in recent years begun to receive at least a limited amount of financial aid from this highly respectable source. (In this connection we will mention only the more prominent general

foundations.) From 1950 to 1953 the Rockefeller Foundation made grants totalling $45,000 to the Parapsychology Laboratory of Duke University. In 1953 the Mellon Charitable Trust of Pittsburgh gave $50,000 to the University of Pittsburgh for parapsychological investigation. In this connection might be listed also the action of the Office of Naval Research of the U.S.A. in its support of a branch of inquiry, the research on pigeon homing, now in its third year at the Duke Laboratory. There have been other grants both here and abroad. The recent establishment of the Parapsychology Foundation of New York is in itself undoubtedly a significant indication of the stage reached by the field.

In view of these various gestures from so wide a range of competent judges, it would seem that the general acceptance of parapsychology must be just a question of time. Perhaps one of the best signs that ground is being gained—and this is a sign which is bound to strengthen the confidence of the research worker—is that now, after seventy-five years of debate over psi, the level of discussion among qualified scholars has at last moved on from the elementary question of whether psi really exists to a consideration of *what psi phenomena mean.* This progress is indicated not only by the reflections of the many professional philosophers who, as already indicated, have taken up the questions raised by parapsychology; there are also leading and representative men from other fields who are entering into these interpretative analyses in an active way. Even those who, like the psychologist, D. O. Hebb, reject the evidence after revealing they do so on metaphysical grounds, really belong to this area of interpretative discussion. It is only a step further into the positivist position of E. G. Boring, who, while accepting the findings, minimizes them as mere collections of statistical discrepancies; and it is only a second step to those who insist on postponing the explanation of psi, as do Roland Walker and C. J. Herrick, until, as they believe will happen eventually, the results harmonize with the now current mechanistic picture of the universe. While they too concede that psi phenomena do occur, and while they do not minimize the importance of the phenomena, they cannot accept the conclusion that psi is not physically explainable. In other words, they accept all the facts but those showing that psi is nonphysical.

It is even a shade better for the parapsychologist to see the bold efforts of leaders in neighboring fields of science or practice attempting to integrate psi with the more familiar knowledge of their own fields of study. To some extent this impulse toward integration has been going on for a long time. Stekel and Freud, in their day, did not look upon telepathy as a function that had to be rejected because it could not be fitted into their conceptions of depth psychology. McDougall considered the evidence of parapsychology as a confirmation of his mind-body hypothesis of interactionistic dualism. Jung has gone perhaps further than anyone in his utilization of the psi researches as the primary, if not the sole, experimental evidence supporting his hypothesis of the acausal association of experience and event which he calls "synchronicity." In the field of biology, Sinnott has repeatedly said that psi must be kept in mind for its possible bearing on the central problem of the nature of the living organism. Hardy has suggested telepathy as a factor in evolution, and Berrill, among others, has kept telepathy in mind in connection with the unexplained direction-finding powers of migratory animals. Eccles, in neurophysiology, has introduced the evidence of parapsychology into the discussion of his theory of the thought-brain relationship. Pascual Jordan has attempted a harmonization of psi with theoretical physics. Finally (although the list is far from complete), Thouless and Wiesner have tied the whole domain of parapsychology into a theory of the psychophysical organization of personality.

Whatever the success of these hypothetical interrelations and applications, the mere impulse to make them, and the serious consideration of psi results which they imply, are greatly encouraging in themselves to the scientific worker in parapsychology. For it is very important to any scientist to have his confidence in the progress of his field of inquiry properly established. This benefit lies partly in the greater freedom it allows him in making bolder forward thrusts of inquiry. It is only from a position of comparative security and confidence that an explorer can reach far over into areas that involve exceptional risks. Uncertainty over the accreditation of what he has already achieved naturally tends to make an investigator excessively conservative. He will be less likely to venture much

if he can secure neither confidence nor recognition. Those who are justly anxious today—as many are—about the comparative state of American science in general or—as few are—about the status of parapsychology would do well to appreciate the psychological aspects of the practice of exploratory research under frontier conditions of inquiry.

In parapsychology there is now a special need to attack larger projects than present research conditions allow. These major problems are waiting partly for the removal of the sense of unrealism that goes with getting too far out ahead and being too isolated on a research line which fellow-scientists do not take seriously. To go too far beyond one's companions can come to seem foolhardy rather than courageous; so recognition or the lack of it is important. Every sane scientist who is breaking into new territory must throw an anxious glance over his shoulder from time to time to see how far ahead of his more conservative brethren he is getting. There are limits of safety involved. But beyond any question, the development of a proper confidence in the ultimate acceptance of his findings *on their merits* will be the best possible stimulus to the research worker in parapsychology.

J. B. R.

BOOK REVIEWS

AN ADVENTURE. By C. A. E. Moberly and E. F. Jourdain. (Edited by Joan Evans.) London: Faber & Faber, Ltd., 1955. Pp. 127. 12*s.* 6*d.*

This is the fifth edition of the account first published pseudonymously in 1911 of what purported to be a psychic experience undergone in 1901 at the Petit Trianon, Versailles, by two academic Englishwomen. Their "researches" into French history convinced them that they had witnessed scenes from the past of Marie Antoinette's day. We have a new editor, but this edition is no more satisfactory than the last four, and its faults, both as regards things done and those left undone, are many.

There are too many inaccuracies. The wretched confusion which has prevailed from the beginning still continues, and is even here and there worse confounded. The editor has failed to supply the elementary needs of an index and some sort of guide through the chaotic "researches," to say nothing of the badly wanted accurate sketch map of the terrain.

There is a more important omission, however. For nearly fifty years critics of weight have attached importance to the fact that the story, as it was given to the public, in the form of two separate and so-called "independent" accounts, had been expanded and amended from the original two manuscripts, and differed from them in many respects. Perhaps as a result of criticism on this score, and despite the authors' refusal to allow mention of these earlier manuscripts in the S.P.R.'s review, they were published (if rather obscurely, in an appendix) in the second, 1913, edition. They have never again appeared, however, and that edition is now unobtainable. The significance of the differences between the earlier and the later accounts has been explained with force and luminous clarity by Mr. W. H. Salter in his "Note on the Evidence of an Adventure." (*Journal of the S.P.R.* No. 656, Jan.-Feb. 1950. Vol. XXXV.)

As Dr. Evans specifically refers to this "Note" it is hard to understand how she can claim that she is divesting her edition (in

which, once again, only the later accounts are published) of "later accretions." It is even harder to follow her when she says that she has checked the *printed text* against the *manuscript* originals, and refers contemptuously to "insignificant divergencies [*sic*]." To make a point of totally innocuous adjustments and discrepancies about which no one has ever raised any question, while ignoring the vital omissions, additions, alterations, contradictions and suppressions which differentiate the second from the first set of documents is indeed to swallow a camel and strain at a gnat!

The edition is characterized by portrait sketches of Miss Moberly and Miss Jourdain written by the editor and affecting to be "deliberately unsoftened and even unflattering." There are two major errors of fact, easily verifiable, in the first nineteen lines, and this is by no means the end of misstatement. Miss Moberly's portrait may be said to be unflattering, but it is not unjust. On the other hand, the study of Miss Jourdain is so partial and incomplete as to be positively misleading. All will admire Dr. Evans' loyalty to the tragically unhappy College Principal whose defects of character ruined her own career and threatened to bring her institution to destruction, but for a biographical note of this kind loyalty is not enough. Moreover, an undeclared "interest" of devoted friendship is highly undesirable in one who presents herself to the undiscerning public as an impartial assessor in a highly controversial subject.

LUCILLE IREMONGER

GLOSSARY

In order to avoid constant redefining of commonly recurring terms in papers appearing in this JOURNAL, the following definitions are submitted for convenient reference.

A simple description of the basic experimental methods and of the statistical procedures to be followed in evaluating ESP (both card tests and drawings tests) and PK may be found in the *Handbook of Tests in Parapsychology* by Betty M. Humphrey (published by the Parapsychology Laboratory, Duke University, Durham, N. C.).

AGENT: In tests for telepathy, the person whose mental states are to be apprehended by the percipient. In GESP tests, the person who looks at the stimulus object.

AVERAGE SCORE: Average number of hits per run.

BT ("Broken Technique"): The clairvoyance technique in which each card is laid aside by the experimenter as it is called by the subject. The check-up is made at the end of the run.

CALL: The ESP symbol selected by the subject in trying to identify a target.

CHANCE: The complex of undefined causal factors irrelevant to the purpose at hand.

CHANCE EXPECTATION = MEAN CHANCE EXPECTATION: The most likely score if only chance obtains.

CHANCE AVERAGE: Mean chance expectation expressed as an expected score, generally in terms of average per run.

CHI SQUARE: A sum of quantities each of which is a deviation squared divided by an expected value. Also a sum of the squares of CR's.

CLAIRVOYANCE: Extrasensory perception of objective events as distinguished from telepathic perception of the mental state of another person.

CR (CRITICAL RATIO): A measure to determine whether or not the observed deviation is significantly greater than the expected random fluctuation about the average. The CR is obtained by dividing the observed deviation by the standard deviation. (The probability of a given CR may be obtained by consulting tables of the probability integral, such as Pearson's.)

CR OF THE DIFFERENCE: The observed difference between the average scores of two samples of data divided by the standard deviation of the difference.

DECK: Twenty-five ESP cards.

CLOSED DECK: An ESP deck composed of five each of the five symbols.

OPEN DECK: An ESP deck made up of the ESP symbols selected in random order.

DEVIATION: The amount an observed number of hits or an average score varies from the mean chance expectation or chance average. A deviation may be total (for a series of runs) or average (per run).

DIE THROW: The elementary unit of data in a dice-throwing test represented by the throwing and reading of a single die, no matter how many dice are thrown at a time.

DISPLACEMENT: ESP responses to targets other than those for which the calls were intended.

BACKWARD DISPLACEMENT: ESP responses to targets preceding the assigned targets (the ones for which they were intended). Displacement to the targets

one, two, three, etc. places preceding the assigned target are designated as (−1), (−2), (−3), etc.

FORWARD DISPLACEMENT: ESP responses to targets coming later than the assigned targets. Displacement to the targets one, two, three, etc. places after the assigned target are designated as (+1), (+2), (+3), etc.

DT (Down Through): The clairvoyance technique in which the cards are called down through the deck before any are removed or checked.

EMPIRICAL CONTROL: An experiment which wholly or partially follows the main experiment with the exception that the conditions are designed to exclude the possibilty of the hypothesis being tested.

ESP (EXTRASENSORY PERCEPTION): Response to an external event not presented to any known sense.

ESP CARDS: Cards, each bearing one of the following five symbols: star, circle, three parallel wavy lines (called "waves"), square, plus.

ESP SYMBOLS: See plate opposite page 1, this JOURNAL, Vol. 1, March, 1937.

EXPECTATION; see CHANCE.

EXTRACHANCE: Not due to chance alone.

FREE MATERIAL: The stimulus objects of experiments in which an unlimited or unspecified range of stimulus objects is employed (as contrasted with methods such as card-calling in which the subject knows that the stimulus object is one of a known range).

FREE RESPONSE METHOD: An ESP test method in which a relatively unlimited range of stimulus objects is used and in which the subject is instructed to express freely as his response any range of thoughts and ideas.

GESP (GENERAL EXTRASENSORY PERCEPTION): A technique designed to test the occurrence of extrasensory perception, permitting either telepathy or clairvoyance or both to operate.

HIGH-DICE TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling eight or more.

LOW-DICE TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling six or less.

MEAN CHANCE EXPECTATION; see CHANCE.

P (PROBABILITY): A mathematical estimate of the expected relative frequency of a given event if chance alone were operative.

PARAPSYCHOLOGY: A division of psychology dealing with those psychical effects which appear not to fall within the scope of what is at present recognized law.

PERCIPIENT: The person who makes the calls in an ESP test.

PK (PSYCHOKINESIS): The direct influence exerted on a physical system by a subject without any known intermediate physical energy or instrumentation.

PRECOGNITION: Cognition of a future event which could not be known through rational inference.

PREFERENTIAL MATCHING: A method of scoring free responses. A judge ranks the stimulus objects (usually in sets of four) with respect to their similarity to, or association with, each response; and/or he ranks the responses with respect to their similarity to, or association with, each stimulus object.

PSI: A general term to identify personal factors or processes in nature which transcend accepted laws. It approximates the popular use of the word "psychic" and the technical one, "parapsychical."

PSI PHENOMENA: Occurrences which result from the operation of psi. They include the phenomena of both ESP (including precognition) and PK.

QD (QUARTER DISTRIBUTION): The distribution of hits in the record page (or in a logical subdivision thereof, such as the set or the half-set) as found in the four equal quarters formed by dividing the selected unit horizontally and vertically.

RUN: A group of trials, usually the successive calling of a deck of 25 ESP cards or symbols. In PK tests, 24 single die throws regardless of the number of dice thrown at the same time.

SCORE: The number of hits made in one run.

TOTAL SCORE: Total of scores made in a given number of runs.

AVERAGE SCORE: Total score divided by number of runs.

SD (STANDARD DEVIATION): Usually the theoretical root mean square of the deviations. It is obtained from the formula $\sqrt{npq}$, in which n is the number of single trials, p the probability of success per trial, and q the probability of failure. (For ESP cards, SD $= 2\sqrt{\text{no. of runs.}}$) Sometimes the SD used is that estimated from the observed variability in the scores.

SD OF THE DIFFERENCE: For both ESP cards and PK tests using dice, the SD of the difference is equal to $\sigma_s \sqrt{1/R_1 + 1/R_2}$ where σ^2 is the SD of a single run and R_1 and R_2 are the number of the runs in the respective samples compared. This gives the SD of the difference for run score averages.

SERIES: Several runs or experimental sessions that are grouped in accordance with a stated principle.

SESSION: A unit of an ESP or PK experiment comprising all the trials of one test occasion.

SET: A subdivision of the record page serving as a scoring unit for a consecutive group of trials, usually for the same target.

SEVENS TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling seven.

SIGNIFICANCE: A numerical result is significant when it equals or surpasses some criterion of degree of chance improbability. The criterion commonly used in this JOURNAL is a probability value of .02 or less, or a deviation in either direction such that the CR is 2.33 or greater.

SINGLES TEST: A PK technique in which the aim of the subject is to try to influence dice to fall with a specified face up.

STIMULUS OBJECT: The ESP card or drawing or other object, some identifying characteristic of which is to be apprehended by the percipient.

SUBJECT: The person who is experimented upon. In ESP tests, most commonly the percipient (though also the agent in GESP and telepathy). In PK tests, any individual whose task it is to influence the objects thrown.

TARGET: In ESP tests, the stimulus object; or in telepathy, the mental state of the agent. In PK tests, the faces of the die (or combination of faces) which the subject attempts to bring up in the act of throwing.

TARGET CARD: The card which the percipient is attempting to perceive (i.e., to identify or otherwise indicate a knowledge of).

TARGET DECK: The deck of cards the order of which the subject is attempting to identify.

TARGET FACE: The face on the die which the subject tries to turn up as a consequence of direct mental action.

TELEPATHY: Extrasensory perception of the mental activities of another person. It does not include the clairvoyant perception of objective events.

TRIAL: In ESP tests, a single attempt to identify a stimulus object. In PK tests, a single throw of the dice or other objects thrown.

HERE AND THERE IN THE LITERATURE

The high possibilities that lie before us should be grasped once for all, in order that the dignity of the quest may help to carry the inquirer through many disappointments, deceptions, delays. But he must remember that this inquiry must be extended over many generations; nor must he allow himself to be persuaded that there are byways to mastery. I will not say that there cannot possibly be any such thing as occult wisdom, or dominion over the secrets of nature ascetically or magically acquired. But I will say that every claim of this kind which my colleagues or I have been able to examine has proved deserving of complete mistrust; and that we have no confidence here any more than elsewhere in any methods except the open, candid, straightforward methods which the spirit of modern science demands.

F. W. H. Myers, *Human Personality*